# On the Microeconomics of Trust

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# PREFACE

Whereas the work in this thesis is my own, it obviously developed and matured within a context of advisors, colleagues, friends and family, to whom I am exceedingly grateful.

First and foremost, I would like to express my most sincere gratitude to my supervisor and mentor Ernst Fehr. I originally began to work for Ernst as an undergraduate student and my first task was to run dozens of experiments. That is where I learned how to conduct an economic experiment properly. I will never forget all those public good games. Ernst introduced me to the emerging and fascinating field of Experimental Economics. To a great extent it was simply by watching Ernst do research that I learned all the necessary tools and skills which eventually lead to the completion of this thesis. As a doctoral thesis supervisor, Ernst was demanding when I needed a push and patient when ideas required time to mature; and he never failed to inspire.

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My time at the Institute for Empirical Research in Economics at the University of

Zurich was of outstanding importance for this thesis. One of the many reasons is that there were so many great researchers around who were always available for exchanging ideas. Such an environment motivates, helps, and puts the fun back into research. Among the many members of the institute, I would like to particularly thank Lorenz Goette, Simon Gaechter, Armin Falk, Christian Zehnder and Daniel Schunk, with whom I frequently interacted, for their support. After moving to the Economics Department at Royal Holloway, University of London, I profited from the department's generosity in allowing me to complete this thesis as well from interesting conversations with my new colleagues. Particularly I wish to mention Dirk Engelmann whose comments have proved to be especially insightful.

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Michael Näf, August 2009

CHAPTER

# ONE

# INTRODUCTION

## 1.1 Why Trust?

Trust is a concept which is familiar to everyone. You do not have to be a scientist or economist to understand how important trust is in our world. Of course, there are many different kinds of trust which we come across in our everyday lives. For instance, there is the basic sense of trust, developed by babies towards their parents and the world - according to the psychiatrist Erik Erikson (Erikson, 1950), the successful development of this kind of trust influences us for the whole of our lives. But there is also the kind of trust which we witness between couples in long standing relationships, where each partner knows the other so well that very little communication is needed. A nod, a sigh, and they trust the other to understand their meaning and react appropriately. We also understand that sometimes trust can be blind, where our willingness to believe in the good of people can lead us to blank out all signs to the contrary, thus making us vulnerable to exploitation. For some people, there is a very basic and powerful trust in god or another higher force, a phenomenon which we often call faith. Self-confidence can also be seen as a form of trust, where we primarily trust ourselves, having faith in our own abilities and worth.

Perhaps it is precisely because we encounter trust in so many different situations, and because it often appears to be lacking of any sound rational basis, that economists have, in an historical perspective, only recently begun to investigate trust as an economic phenomenon. The main relevance attributed to trust in this discipline is founded on the identification of trust as an important, if not vital, element of economic transactions. It can be as simple as a purchase: How can I be sure that once I hand over my money, you will not simply run away and keep it, without ever passing on the goods I paid for? As long as the two parties of the transaction know each other and are perhaps bound

in a web of mutual obligations of kinship and honour, it is easy to trust the other. However, nowadays only a small minority of economic transactions is completed in such circumstances. In the vast majority of cases, the participants of the transaction know little of each other. Here, it is the state and institutions which fill the gap and trust is placed in the rule of law rather than in individuals.

Economic transactions thus benefit greatly from legal security and certainty, and this has therefore been studied by economists around the world. And yet, whereas trusting the state to guarantee economic obligations enables transactions, this trust may come at high costs. On the one hand, there are the costs associated with the upkeep of a legal system which is able to provide such guarantees, without becoming totalitarian in its scope of control. On the other hand, and most importantly, there are the costs of spelling everything out so as to make it legally enforceable. If we imagine for a moment a society where no-one really trusts anyone else, but rather everyone places all their trust in the state and rule of law, then even the simple act of buying a loaf of bread from the bakery would require a complicated, fool-proof contract. It is here that the importance of trust in strangers becomes apparent. If I do not know the baker, but do not generally mistrust anyone I do not know well, then I can complete this transaction without the need of safeguarding against all manner of occurrences however unlikely they might be. Thus, trust in strangers proves to be a lubricant for social relations. It is this kind of trust, trust in strangers, which the essays in this collection focus on.

In situations of social uncertainty, trust helps to form relations. It reduces the transaction, monitoring and sanctioning costs of ordinary spontaneous relationships (Arrow, 1974; Putnam *et al.*, 1993)[1]. Empirical indications for the relevance of trust can be found in the fact that trust is not equally distributed throughout the population, nor is its distribution the same or even similar across countries. On the contrary, seminal papers by Knack and Keefer (1997) and La Porta *et al.* (1997) have not only analysed the uneven distribution of trust between countries but have also demonstrated the relation of trust to important economic variables such as growth and inflation in those countries. Further empirical evidence for the importance of trust has only recently appeared, during the last decade, and includes explorations of trust in relation to trade and foreign direct investment (Guiso *et al.*, 2009), stock market participation (Guiso *et al.*, 2004, 2008), investment decisions of venture capitalists (Bottazzi *et al.*, 2008) and tax compliance (Scholz and Lubell, 1998).

[1]Trust has also been explored as part of what constitutes social capital, following Robert D. Putnam's 'Bowling alone', which has breached the boundaries of academia (Putnam, 2000). The extent of the debate on social capital's impact can perhaps be seen in that even politicians and leaders of the World Bank, the White House and the German Chancellery are nowadays working on strengthening the sense of community and thus individuals' trust in each other.

The ascertainment of the importance of trust for economic and social life led experimental economists to study trust since the early 1990s. As economists are mostly concerned with actual behaviour, the introduction of a behavioural measure for trust started a whole wave of new studies. On July 13, 1993 Berg *et al.* (1995) conducted the first session of the investment game which has become the standard design for measuring trusting behaviour. Since then, over a hundred trust games have been run by many different researchers and the article by Berg *et al.* (1995) has been cited more than 370 times according to the Social Science Citation Index and more than 1,200 times according to Google Scholar[2]. These experimental studies covered numerous topics starting from socio-economic determinants of trust (e.g. Bellemare and Kroeger, 2007; Croson and Buchan, 1999; Sutter and Kocher, 2007), to trust and discrimination (e.g. Bornhorst *et al.*, 2009; Burns, 2006; Cardenas *et al.*, 2008a), to communication and trust (e.g. Ben-Ner *et al.*, 2007; Meidinger *et al.*, 1999) across methodological questions (e.g. Burks *et al.*, 2003; Casari and Cason, 2009; Johansson-Stenman *et al.*, 2005a) to biological determinants (for an overview see Fehr, 2009) such as oxytocin (Kosfeld *et al.*, 2005) and vasopressin (Stanton, 2007).

In section 1.3 I will describe in detail precisely how we implemented the trust game for the research presented in this collection. But first, let us carefully consider the meaning of the term 'trust' and provide a definition.

## 1.2 Trust: Easy to Define ...

What is trust? Many studies which analyse trust do not provide any kind of definition, or if they do, it is striking how varied the definitions provided are. The consequence is confusion. Statements on trust might appear to be conflicting or even contradictory superficially, but as they have different starting points, it is near impossible to actually compare like for like, thus opening the way for many misunderstandings (Hardin, 2002). In order to avoid such confusion and to guide our thinking, in the following we shall make clear what we mean by trust. We largely follow James Coleman's concept of trust (Coleman, 1990) which is well suited for behavioural sciences such as economics. Although not very widely known - Coleman is more often read in sociology these days - referring to him in relation to trust is by no means exotic. In fact, Berg *et al.* (1995) also referred to Coleman when they first introduced the trust game. The definition is easily summarised. In Coleman's perspective the following two points characterise the action of placing trust. On the one hand it implies that the truster freely transfers assets to another person, without controlling the actions of that other person or having the

[2]Accessed on August 11, 2009

possibility to retaliate. On the other hand there must be a potential gain in order to have an incentive to trust. The incentive is such that if the other person is trustworthy, the truster is better off trusting than not having trusted, and worse off if the other person does not fulfil the trust placed in him/her. Going beyond Coleman's simple definition, we feel that for clarity's sake, it is necessary to specify that 'worse off' can be understood in an absolute sense where the truster ends up with less assets than initially, however it can also be understood relatively where 'worse off' is seen in relation to the other person's assets. Coleman further adds that in his definition there is no monetary incentive for the second-mover to reward the first-mover's trusting behaviour with trustworthiness. Note that in this concept, trust is considered as a form of behaviour rather than as a personal characteristic or personal trait. Following this concept, the use of experimental methods to measure trust suggests itself (Berg *et al.*, 1995).

In the following I will introduce the specific design of my experimental measure of trust, which I will extensively use in the following three essays.

## 1.3 . . . Difficult to Measure!

We used an adapted version of the design by Berg *et al.* (1995). The design was kept simple. There are two players each endowed with 10 Euros. They do not know each other and will never know the identity of the other player. The first-mover decides on how many of his/her 10 Euros he/she would like to transfer to the other player. Every Euro transferred to the other player is doubled by the experimenters. Then it is the turn of the other player, the second-mover. Like the first-mover, the second-mover decides how many of the 10 Euros he/she transfers to the first-mover. Again the transfer is doubled by the experimenter. After the decision of the second-mover, the game is over and both players get paid according to the outcome of the game. Thus, the only difference between the decisions of the first and the second mover is that the second-mover already knows what he/she will receive from the first-mover while deciding what to give, whereas the first-mover has to take a risk. That is why we label the first-mover decision a trusting decision, which is a subset of risky decisions. The difference is that in a trusting decision the risk taken depends on the actions of another human. This design is used in all the studies presented in this thesis[3].

We combined the trust game with the German Socio-Economic Panel Study (SOEP), a household panel representative for Germany (Wagner *et al.*, 2007). A randomly chosen subset of respondents of the SOEP participated in the experiment. The SOEP is an interviewer based study, which means that the experiment was conducted by inter-

[3]The exception is chapter 2 where we vary the design slightly to test the sensitivity of the design.

viewers within the homes of the respondents. After the normal SOEP questionnaire was completed, the interviewer handed over the written instructions to the participants. The interviewers were briefed to give, if needed, detailed information to the participants before they made their decisions. Then the participants were asked to privately make their decision and put the decision sheet into an envelope and close it. Thus the interviewers did not know and will never find out how the participants decided. Feedback and a check for the amount earned were mailed to the subjects a few weeks later, together with a thank-you letter.

The are several advantages of including behavioural experiments in large representative surveys. First, we have the opportunity to measure behaviour rather than intentions. Second, this approach allows us to simultaneously analyse the influence of a large variety of personal characteristics on trusting behaviour. Third, in contrast to many studies based on experiments conducted with student populations, we have the opportunity to analyse trusting behaviour of the general populace of a country. This has the advantage that the heterogeneity of our subjects' behaviour and characteristics is much larger than within a student population.

## 1.4 Three Essays on Trust

Given the importance generally attributed to trust - confirmed by the studies mentioned - it is surprising how little we actually know about it. Research on trust spans a range of disciplines; but even within a single discipline such as economics, there is little that is agreed upon. Disagreement starts with the definition of trust, extends to methodological questions of how best to measure trust as well as to what explains differences in trust, and does not end with what drives changes in trust. The disagreement can mainly be attributed to a lack of studies and/or suitable data to analyse the questions raised.

In light of the ambiguities surrounding not just the definition of trust, but also the question of how to measure trust (as described in chapter 2), any empirical research on trust needs to carefully review the different methods and select the one most suited for the purpose. However, little is yet known about how the different measures of trust relate to each other. Accordingly, this is one of the questions that this collection's first essay 'Can We Trust the Trust Game? A Comprehensive Examination.' addresses. In this essay I analyse the gaps that remain in our understanding of what exactly is measured by the different experimental and survey measures of trust and how they relate to each other. The relation between experimental and survey measures of trust is a topic which many papers have touched upon. However, they all have either comprehensive survey measures or extensive experimental measures but never both.

In contrast, our study examines survey and experimental measures, both comprehensively and simultaneously. Using nationally representative data, we are thus able to demonstrate that the commonly used trust game measures trust in strangers, but is not related to other standard survey measures of trust. We also demonstrate that the trust game is robust to a number of interferences. Further, we show that selection for the experiment can be a problem if people are unfamiliar with the situation and that students exhibit higher trust than the general population – a result which needs to be taken into consideration by researchers working with student subjects only. Inspired by criticism of the widespread trust question used in many surveys, we created a new, improved survey trust scale consisting of three short statements. In the essay we demonstrate that our new scale is a valid and reliable measure of trust in strangers. The analysis shows that the experimental measure correlates with the survey measure of trust in strangers but not with trust in institutions nor with trust in known others. The survey measure of trust captures mainly the expectations part of the decision to trust. The survey and the experimental measure correlate similarly with related factors such as risk aversion, entrepreneurship and share-holding.

We conclude that the experimental measure of trust is a robust and valid measure which does not refer to trust in a general sense, but specifically to trust in strangers.

Having established a definition of trust in this introduction as well as a sense of the advantages and peculiarities of an experimental methodology, in the first essay, we next turn to some topical questions.

In the second essay 'Decomposing Trust: Explaining National and Ethnic Trust Differences', I focus on the striking differences in aggregate levels of trust (by individuals) which we find between nations. This is especially intriguing as national differences in trust may well be at the core of understanding differences in economic developments and between changes of economic systems. Unlike survey studies of trust, which have revealed large differences in the levels of trust in different countries, no study has yet investigated cross-country differences in trusting *behaviour*. This might be due to a lack of resources needed to collect this kind of data. Previous studies either investigated trusting behaviour representatively for a single country or a specific region or city (Bellemare and Kroeger, 2007; Cardenas *et al.*, 2008b; Johansson-Stenman *et al.*, 2005b), or compared trusting behaviour of non-representative samples across countries (Buchan *et al.*, 2002, 2006; Croson and Buchan, 1999; Csukas *et al.*, 2008; Willinger *et al.*, 2003). In contrast, in this essay we measure trusting behaviour in two nationally representative samples of the United States and Germany for the first time. We detect a large trust gap: people in the United States trust strangers much more than do people in Germany. Based on variables suggested by economic theory, we can explain almost the

entire trust gap. People in the United States are less risk and betrayal averse. Moreover, they expect to achieve a high payoff as well as equality between truster and trustee at much higher trust levels than Germans. Overall, socioeconomic variables and income explain only 7% each of this trust gap while preferences and belief-variables suggested by economic theory explain 91% of the gap. Within the United States Caucasians trust more than Latinos and Latinas, who in turn trust more than African Americans. These differences are similarly explained by the same variables. Therefore we conclude that risk and social preferences and belief variables constitute important explanatory factors and deserve increased attention such as inclusion in future survey research.

In the third essay 'Mistrust and Betrayal: a Vicious Circle', I analyse changes in trusting behaviour. This is interesting since little is known about the temporal dynamics of trusting behaviour. On the basis that trust in strangers is important for economic and social relations, we study the forces that drive the changes in individuals' levels of trust. From theory we expect that positive and negative experiences change trusting behaviour. However, no empirical study has yet investigated whether and how this happens. In this paper, we are able to establish a strong causal link from past experience of untrustworthiness to reduced trusting behaviour, measured by a repeated trust game over the period of one year. We determine that this causal link mainly functions by way of changed beliefs, and is moderated by social preferences. Experience of trustworthiness, in contrast, does not alter trusting behaviour. Furthermore, we investigated if an increase in the experience of trust promotes trustworthy behaviour. We show that there is indeed a causal link here, too. We conclude that negative experience in relation to trust might kick-start a vicious circle thus eroding overall levels of trust. Further research is needed to improve understanding of the factors that increase trusting behaviour.

CHAPTER

**TWO**

# CAN WE TRUST THE TRUST GAME? A COMPREHENSIVE EXAMINATION

Joint with Jürgen Schupp

## 2.1 Introduction

In surveys like the General Social Survey (GSS) or the World Values Survey (WVS), trust is measured with the statement '*Generally speaking, would you say that most people can be trusted or that you can't be too careful in dealing with people?*' This measure of trust has been criticised and its behavioural relevance has been called into question.

The first systematic study of the relation between survey and behavioural measures of trust was reported by Glaeser *et al.* (2000). They investigated whether behaviour in a trust game is correlated with this standard survey measure of trust. They find that the above question is not correlated with trusting behaviour. This result has been replicated in several other studies (e.g. Ashraf *et al.*, 2006; Burks *et al.*, 2003; Ermisch *et al.*, 2009; Gächter *et al.*, 2004; Haile *et al.*, 2008; Holm and Nystedt, 2008; Johansson-Stenman *et al.*, 2005b). No correlation either was found by Eckel and Wilson (2000) and Eckel and Wilson (2002) using a slightly different trust scale. However, other studies have found a significant correlation between the survey and the experimental measures (e.g. Bellemare and Kroeger, 2007; Cox *et al.*, 2009; Holm and Danielson, 2005; Sapienza *et al.*, 2007; Vyrastekova and Garikipati, 2005). Based on the previous research one cannot conclude whether the GSS question is behaviourally relevant in the sense that it correlates with the behaviour in the trust game.

What are the reasons for these conflicting results? Are the experiment and the survey measures both valid and reliable measures of trust? Concerning survey measures, several

studies have revealed that the GSS question is neither a valid nor a reliable measure of trust (Reeskens and Hooghe, 2008). The question is rather imprecise, the possible answers are not mutually exclusive, and only one item is not considered to be a reliable measurement (e.g. Glaeser *et al.*, 2000; Miller and Mitamura, 2003; Yamagishi *et al.*, 1999). Concerning the experimental measure, little is known about its sensitivity and validity in large and heterogeneous populations.[1] Against this background, it is no surprise that there is no clear relationship between survey and experimental measures of trust.

The aim of this article is to connect the survey measures and the experimental measures of trust. In particular, we aim to show that survey and experimental measures can be connected in a large representative survey. Since the experimental measure might capture a very specific dimension of trust, we created a new survey measure of trust that, on the one hand, takes recent criticisms of the GSS question into account, and on the other, measures the same dimension of trust as the experiment.

To avoid confusion we have to clarify first what we mean by the word 'trust'. We largely follow James Coleman's concept of trust (Coleman, 1990). From his perspective, the following two points characterise the action of placing trust. On the one hand, trust implies that the truster freely transfers assets to another person, without controlling the actions of that other person or having the possibility to retaliate. On the other hand, there must be a potential gain in order to have an incentive to trust. The incentive is such that the truster is better off than not having trusted if the other person is trustworthy, and worse off if the other person does not merit the trust placed in him/her. Note that in this concept, trust is considered a form of reflected behaviour rather than as a personal characteristic or personal trait.

Our new survey measure is more precise than the GSS question on what dimension of trust is measured. We frame and focus the response on trust in strangers. We have used this new survey measure in the German Socio-Economic Panel (SOEP) study and several independent studies that are all representative for the German population. In order to distinguish the newly developed trust questions from the GSS question, we refer to them as 'SOEP-trust'. A factor analysis shows that SOEP-trust (trust in strangers) measures a different dimension of trust than questions on trust in institutions and trust in known others. Further, we show that SOEP-trust is a valid and a sensitive measure of trust. Concerning the latter, we find that SOEP-trust is correlated with social desirability and the position in the survey. Furthermore, trust is moderately stable over three weeks. This has implications for the use of the survey question in international comparisons and over time.

[1] Most experiments are run with students as subjects

The design of the simplified trust game is as follows. Two players are each endowed with 10 euros. The first mover decides how many of his or her 10 euros he or she would like to transfer to the second mover. The transferred amount is doubled by the experimenters. The second mover then gets to know the first mover's transfer and then decides him or herself about the back-transfer. As with the first mover, the second mover can transfer any amount between zero and ten euros. The second mover's transfer is doubled as well. Then the game is over and both participants are paid by a cheque. In order to distinguish the experiment from other trust measures we refer to it as 'EXP-trust' in the following.

Our implementation of the experiment in a large survey has several advantages over an ordinary laboratory experiment. With our survey measure of trust, we are able to directly check whether only highly trusting people decide to participate in the trust game. Since this is a panel study, we can additionally check whether less trusting people are more likely to leave the panel in the future. Further, we can compare students and non-students in the same experimental setting (design and procedure).

Another advantage of the combination of survey and experiments is that we can use the information in the survey to validate EXP-trust. We thus analyse whether the decision to trust is influenced by preferences, and expectations, as postulated by economic theory. We also assessed the sensitivity of the experimental design to a social desirability bias, the stake size, and the available strategy space.

We find that EXP-trust is surprisingly robust and also not subject to a social desirability bias, and not dependent on the exact stake size or on the size of the strategy space. Furthermore, we find that for subjects who are familiar with the interview situation (i.e., through previous participation in a panel study), selection into the experiment is not subject to their level of trust. In contrast, for subjects who have not previously been part of a panel study, more trusting people are more likely to participate in the experiment. And contrary to previous research, we find that students due to their higher education are more trusting than non-students, which has consequences for the generalisability of experimental results from students to the general population.

Finally we analyse what kind of trust the experiment actually measures. We find that EXP-trust measures people's trust in strangers, but not their trust in institutions or in known others. That is, EXP-trust is significantly correlated with the newly developed SOEP-trust measure. Both, SOEP- and EXP-trust, correlate similarly with other variables such as education, income, risk aversion, reciprocity, being an entrepreneur and shareholder, which confirms that they measure the same underlining concept. In more detail, SOEP-trust mainly (but not exclusively) measures the expectation part of the decision to trust.

Thus, on a representative level for Germany, we show that survey and experimental measures of trust are connected in the way that the trust game measures a specific dimension of trust, that is, trust in strangers.

In the following, we first analyse the survey measure of trust (SOEP-trust), and in the second part analyse the experimental trust measure (EXP-trust). In the third part, we combine these two measures and analyse their similarities and differences.

## 2.2 Using Surveys to Measure Trust

In this section, we propose a new measure of trust in strangers and analyse its sensitivity, reliability, and validity. In particular, we show that trust in strangers measures a specific dimension of trust that is distinct from other dimensions like trust in institutions or trust in known others. We implement this entire analysis in the framework of the German Socio-Economic Panel (SOEP). The SOEP is a household panel that contained 22,611 individuals in 12,061 households in the year 2003, comprising a representative sample for Germany (Wagner *et al.*, 2007). Additionally, we conducted five accompanying studies (AS), one each year from 2002 to 2006. These accompanying studies all have a randomly drawn sample of 400 to 1,000 observations of the German population and are thus representative for Germany as well. Together these data sets provide a suitable tool to assess survey questions in a large heterogeneous population. In Appendix 2.A (Table A.1) we list all the surveys used and give an overview of which survey we used to implement the different variations of the study design. In Appendix 2.B, we discuss whether our results can be viewed as representative for trust.

### 2.2.1 SOEP-Trust — A New Measure of Trust

The General Social Survey (GSS) measure of trust, together with the quite similar question in the World Values Survey (WVS)[2] is probably the most widely used question to measure trust in surveys: *Generally speaking, would you say that most people can be trusted or that you can't be too careful in dealing with people?* The possible answers are: *Most people can be trusted, Can´t be too careful*, or *Do not know.*

This question measures people's expectations of others' trustworthiness. Based on our concept of trust, expectations about other people's trustworthiness is an important factor in deciding whether one decides to trust or distrust. The advantage of this question is that the same question is used over time and space, thus allowing a wide array of different analyses. However, it has been criticised that the respondents have the choice between

[2]Generally speaking, would you say that most people can be trusted or that you need to be very careful in dealing with people? Most people can be trusted OR Need to be very careful.

trust and caution and not between trust and distrust or between cautious and incautious behaviour (for a review, see Yamagishi *et al.*, 1999). Although trust and caution are difficult to disentangle, it is important to measure them separately, since trust and caution are not necessarily mutually exclusive. The interpretation of the GSS question can therefore differ widely among different societies (e.g. Gabriel *et al.*, 2002). Miller and Mitamura (2003) showed, for example, that Japanese students are more trusting than Americans measured with the above question from the GSS. Measuring trust and caution separately, they find, however, that American students are more trusting than Japanese students but at the same time also more cautious. These differing results clearly demonstrate the problems for the interpretation of the above question.

Based on this evidence, we decided to create a new measure of trust using the German Socio-Economic Panel. We split the GSS question up into two parts. On the one hand, we asked people to what extent they agree with the following two statements:

- *In general, you can trust people.*
- *Nowadays, you can't rely on anybody.*

The possible answers on a four point rating scale were 'disagree strongly', 'disagree somewhat', 'agree somewhat', or 'agree strongly'.

Another criticism of the GSS question is that answers may differ significantly depending on whether people understand 'most people' in the question as meaning acquaintances or strangers (Reeskens and Hooghe, 2008). We therefore let people rate their agreement with two further statements about trust and caution, in which we clearly state that trust towards strangers is meant and not towards family or friends:

- *How much do you trust strangers you meet for the first time.*
- *When dealing with strangers, it's better to be cautious before trusting them.*[3]

The possible answers on a four-point rating scale were either 'no trust at all', 'little trust', 'quite a bit of trust', and 'a lot of trust' for the first question and 'disagree strongly', 'disagree somewhat', 'agree somewhat', or 'agree strongly' for the second question. These four statements constitute our new measure of trust in strangers. To distinguish it from GSS trust, we will call it SOEP-trust in the following.

The emphasis on trust in strangers takes into account that trusting people in general is a very heterogeneous concept. To test whether SOEP-trust measures trust in strangers specifically, we let people rate other statements on trust in different institutions like the

[3]The second part of the question emphasises the timing of the decision to trust. Thus, survey respondents could express their agreement with the proposed sequence of actions. The question does not imply a contrast between trust and cautiousness.

police or the government and in acquaintances like friends and family. The list of items can be seen in Table 2.1. People could answer on the scale from 'no trust at all', 'little trust', 'quite a bit of trust', to 'a lot of trust'. Because of the multidimensionality of trust, these items are expected to measure different aspects of trust. A principal component analysis over all the trust items can show us how many dimensions these items measure[4]. We find that these items represent three distinct components.

Table 2.1: Dimensions of trust

| | Factor 1 Trust in institutions | Factor 2 Trust in strangers | Factor 3 Trust in known others |
|---|---|---|---|
| **How much trust do you have in...** | | | |
| ... parliament | **0.742** | 0.173 | -0.006 |
| ... public authorities | **0.715** | 0.139 | 0.107 |
| ... the European Union | **0.686** | 0.163 | 0.004 |
| ... courts | **0.665** | 0.085 | 0.092 |
| ... large companies | **0.581** | -0.003 | 0.033 |
| ... churches | **0.460** | 0.218 | 0.193 |
| ... schools and the educational system | **0.564** | 0.088 | 0.193 |
| ... press | **0.550** | 0.076 | 0.081 |
| ... labour unions | **0.493** | 0.015 | 0.028 |
| ... police | **0.584** | -0.015 | 0.273 |
| ... your own family | 0.070 | -0.051 | **0.647** |
| ... neighbours | 0.115 | 0.165 | **0.716** |
| ... friends | 0.045 | 0.145 | **0.695** |
| ... strangers | 0.183 | **0.636** | 0.091 |
| **To what extent do you agree or disagree?** | | | |
| In general, you can trust people. | 0.155 | **0.647** | 0.268 |
| Nowadays, you can't rely on anybody. | -0.106 | **-0.666** | -0.229 |
| It's better to be cautious before trusting strangers. | -0.050 | **-0.685** | 0.154 |

*Notes.* Factor analysis using the principal components factor method and an orthogonal varimax rotation. The table reports the rotated factor loadings for the three factors with an eigenvalue larger than 1.
*Sources.* AS02, AS03, AS04, AS05, and AS06 with a total of 3,180 observations.

Table 2.1 reports the factor loadings of the different items. The bold numbers indicate the component to which each item belongs. Each component has a straightforward interpretation. The first factor can be interpreted as trust in institutions, the second factor as trust in strangers, and the third factor as trust in known others. The second component consists of all the items of SOEP-trust and can thus be interpreted as 'trust

[4]A principal component analysis uncovers the underlying pattern in the data, such that the variance of the data is best described.

in strangers'. This clearly shows that SOEP-trust measures the specific trust people have in strangers. It can clearly be distinguished from trust in institutions and known others. For each of the three dimensions we calculated count indices[5]. The reliability of these scales measured by Cronbach's alpha is quite good. It is 0.82 for the index on trust in institutions, 0.62 for trust in known others, and 0.66 for SOEP-trust. Having introduced these three measures of trust we are interested how sensitive these indicators are.

### 2.2.2 Sensitivity of SOEP-Trust

In this section we assess the sensitivity of SOEP-trust. We check whether the position in the survey matters, whether there is a social desirability bias, and whether SOEP-trust is a stable and reliable measure.

#### Position in the Survey

We varied the position of the items of SOEP-trust in the AS06. Subjects were randomly divided into two groups. In one group, the trust questions were asked early in the questionnaire (number 33 out of 118 questions) and in the other group towards the end (question 93). We compare the ranking and the variance of this variable. The latter is clearly not dependent on the position in the survey (Levene's robust test statistic for the equality of variances: $F(1, 1039) = 0.169$ $P > 0.681$). However, the ranking is affected. Respondents who were asked the questions late in the survey exhibit more trust in strangers than people who answered the question early in the questionnaire (Table 2.2). Since the four preceding questions were the same in both situations, this effect is not likely to be driven by different preceding questions but by the position in the survey. Although the size of the effect is rather small, this suggests that to compare trust across time or space, the items have to have a similar position within the survey.

#### Social Desirability

We measure social desirability by the Balanced Inventory of Desirable Responding (BIDR) developed by Paulhus (1991). This is well-established survey tool in which respondents rate several statements. Based on this, Winkler *et al.* (2006) developed a

[5]The count indices are the mean answer of the non-missing items that load highest on each of the three dimensions. The bold numbers indicate which item loads highest on which dimension. The value for a person is calculated as soon as at least two items per component are non-missing. For the count index SOEP-trust we additionally made sure that at least one caution item and one trust item was included. For the count index trust in known others, we additionally included an item about trust in co-workers. This item was not included in the factor analysis, since only people with a job are asked. An inclusion would thus exclude a major share of the population.

Table 2.2: Position of the trust questions within the survey

| **To what extent do you agree or disagree?** *(percentage agreeing)* | **Position in survey** **early** | **late** | **Wilcoxon rank-sum test (p)** |
|---|---|---|---|
| - In general, you can trust people. | 72.8% | 73.4% | $> 0.298$ |
| - Nowadays, you can't rely on anybody. | 36.3% | 32.3% | $< 0.073$ |
| - It's better to be cautious before trusting strangers. | 89.4% | 88.2% | $< 0.023$ |
| Count-index: SOEP-trust | **early < late** | | $< 0.017$ |

*Sources.* AS06 with a total of 1,033 observations.

short version that is suitable for large surveys in the general population. The BIDR has two dimensions. One is called 'self-deceptive enhancement' and captures a tendency to see reality in a more optimistic way than justified. This self-deception is not thought to be conscious. The other dimension is called 'impression management' and measures the degree to which a person consciously tries to construct a favourable representation of themselves. Since trust is desirable in society, trust questions are found to be correlated with scales of social desirability (Rotter, 1967).

We find that the dimension 'impression management' is significantly and positively correlated with the survey measures SOEP-trust (Spearman's rank correlation: $\rho = 0.11$), trust in known others ($\rho = 0.15$) and trust in institutions ($\rho = 0.14$). The correlations of survey trust measures with 'self-deceptive enhancement' are quite low and only significant for SOEP-trust ($\rho = 0.07$, $p < 0.051$) and trust in known others ($\rho = 0.08$, $p < 0.036$). The correlation with trust in institutions is not significant ($\rho = -0.00$). Given that these correlations indicate an association between survey measures of trust and social desirability, it would be interesting to see if the correlations are dependent on the interview mode. In AS04 in which we collected information on respondents' social desirability measures, this is not possible since all interviews where oral interviews. In the SOEP however, different modes are used to collect data. It would be an indication for the importance of the interview mode concerning social desirability effects, if the mean trust varied between interviews that were conducted orally and those that were conducted by post. However, we do not find any difference between these modes in SOEP-trust (Mann-Whitney test: $z = 1.26$, $p > 0.207$), which suggests that the above reported correlations do not depend on the presence of an interviewer.

We conclude that people who are subject to a social desirability bias are likely to overstate their trust in strangers, known others, and institutions - and this appears to be independent of the presence (or not) of an interviewer. This calls into question the validity of these measures of trust and raises doubts about the feasibility of comparisons

across space and time.

**Stability and Reliability**

We assessed the stability of SOEP-trust by repeating core questions of the AS05 six weeks later with a sub-sample (n = 193). If trust measured in a survey is a stable measure, we expect a correlation close to one. We find that SOEP-trust is a moderately stable measure since the Spearman's rank correlation coefficient between the two time periods is 0.48[6]. The trust level did not change for one-third of the participants, increased for 37%, and decreased for 30%. Since trust is moderately stable over time, the reliability of SOEP-trust cannot be assessed through a simple correlation. Instead we used a composite reliability test according to Raykov (2004), which tests whether the same single factor underlies a certain set of variables in two points in time. A structural equation model approach is used to calculate the reliability coefficient $\rho$. We find that it is 0.81. This shows that our measure of SOEP-trust is a reliable measure of trust. In sum, SOEP-trust is moderately stable across time, and it does prove to be a reliable measure of trust.

### 2.2.3 Validity of SOEP-Trust

In the previous section we showed that our new survey measure of trust has a high level of reliability but is sensitive to the position and also has a social desirability bias. In this section, we assess the validity of the new measure in two different ways. First, we used a survey measure of past trusting behaviour (Glaeser *et al.*, 2000). Relying on self-reports, we asked people: 'How often do you...'

- ...lend personal possessions to your friends (CDs, books, your car, bicycle etc.)?
- ...lend money to your friends?
- ...leave your door unlocked?

Respondents' answers were either 'never', 'infrequently', 'sometimes', 'often', or 'very often'. A factor analysis confirms the one-dimensionality of these three items. We interpret this as past trusting behaviour indicator. The reliability of these items in a count index is 0.56. A first indication that the new trust measure is valid stems from the fact that it correlates significantly with past trusting behaviour (Table 2.3: Spearman's $\rho = 0.17$). Concerning the other two dimensions, we find that trust in known others

[6]The items of SOEP-trust are correlated as follows: 'In general you can trust people' with 0.41, 'Nowadays, you can't be too careful' with 0.45 and 'It's better to be cautious before trusting stranger' with 0.34.

correlates significantly with past trusting behaviour (Spearman's $\rho = 0.08$) whereas the correlation with trust in institutions is essentially zero and not significant. The latter is not surprising since the past trusting behaviour has little to do with trust in institutions.

Table 2.3: Correlations between different concepts of trust in surveys

| | **self-rep. trusting behaviour** | **ESS** | **NEO-A1** | **GSS** |
|---|---|---|---|---|
| SOEP-trust | 0.17** | 0.47** | 0.55** | 0.53** |
| - In general you can trust people. | 0.10** | 0.37** | 0.49** | 0.47** |
| - Nowadays, you can't rely on anybody. | -0.15** | -0.34** | -0.40** | -0.39** |
| - It's better to be cautious before trusting strangers. | -0.13** | -0.19** | -0.31** | -0.48** |
| - Trust in first-time met stranger | 0.08** | 0.34** | 0.33** | 0.22* |
| Trust in known others | 0.08** | 0.29** | 0.31** | 0.34** |
| Trust in Institutions | 0.02 | 0.28** | 0.31** | 0.29** |

*Notes.* Spearman's rank correlations. The table reports the correlation coefficients. °, *, ** denote statistical significance on the 10%, 5% and 1% level, respectively.
*Sources.* AS02, AS03, AS04, AS05, and AS06 and the data on the GSS trust question was collected in a separate student survey at Royal Holloway, University of London in June 2008.

A second way of assessing the validity of the new trust measure is to compare it to other established measures of trust. Apart from the formulation in the World Values Survey, we used two other well-established measures of trust, namely the question from the European Social Survey (ESS) and the original 10 items of the dimension 'agreeableness' from the NEO-PI-R that measures trust. The ESS-trust question simply asked people on an 11-point Likert scale how much trust they have in others. As can be seen in Table 2.3, this ESS-trust correlates highly ($\rho = 0.47$) with our new measure of trust. A similar correlation of 0.55 can be observed between the new measure of trust with the trust factor of the NEO-PI-R and of 0.53 with the GSS question.

From this we conclude that SOEP-trust is a valid measure of trust in strangers, and is well connected to existing survey measures of trust.

## 2.3 Using Experiments to Measure Trust

In this section we assess the sensitivity and validity of EXP-trust in a common framework. Despite its frequent use in economic research, we are not aware of any other study that analyses the sensitivity and validity of the trust game in depth and at the same time in the same framework. Furthermore, we have chosen to run the experiment with a representative sample in order to compare the experiment directly with representative

surveys. Since not all randomly selected persons agree to participate in an experiment, it is important to check whether results from the experiment can be generalised to a whole population. That is why we start by analysing a possible selection effect, then analyse its sensitivity and validity.

### 2.3.1 Design of the Experiment

The design of the experiment to measure trust is based on the investment game introduced by Berg *et al.* (1995). Two players anonymously interact with each other in the following way. The first mover gets an endowment of 10 points and can transfer 0 to 10 points to the second mover. Every point that is transferred is doubled by the experimenters. The second mover also gets an endowment of 10 points. After receiving points from the first mover, he/she decides on how much of the endowment to transfer back to the first mover (0 to 10 points). As with the first mover's transfer, the back-transfer by the second mover is doubled by the experimenters. After the second mover's decision, the game ends and the subjects are paid their income in euro (1 point equals 1 euro) by cheque sent a few days later.

This design was developed by Fehr *et al.* (2002b) and it was implemented in the SOEP 2003 and in the AS02. In the other two accompanying studies, we used a small variation of the design. In the AS03, we implemented the strategy method for the second mover. In order to keep the game simple, we restricted the options of both players to transfer either 0, 5, or 10 points to the other player. In the AS04, we removed the option to transfer half of the endowment to the other player by allowing them to transfer only 0, 2, 4, 6, 8, or 10 points to the other player. In the SOEP 2003 we implemented a high-stakes treatment that used an exchange rate of 1 point to 10 euros for a small part of the sample (117 out of 1,432). Thus, both players had an endowment of 100 euros. The participant's instructions for the different games can be found in the appendix 2.E.

The Nash equilibrium of this trust game can be described as follows. If we assume that both players are rational and selfish and that this is common knowledge, neither one of the players ever transfers a single point to the other. We can relax the assumption that both players are selfish and instead assume that both players are inequality-averse as described by Fehr and Schmidt (1999). If the second mover is inequality-averse enough, his/her back-transfer is equal to the first mover's transfer. If the second mover's inequality-aversion is not common knowledge, the first mover's transfer depends on his/her belief about the probability that the second mover will equalise the inequality. If this belief is above a certain threshold, the first mover transfers the whole endowment and otherwise nothing at all. One can show that the more inequality-averse the first mover, the higher this threshold is. Another less strict assumption is that players have a

preference for reciprocity instead for complete selfishness (Dufwenberg and Kirchsteiger, 2004; Falk and Fischbacher, 2006). With this assumption, the second mover's back-transfer increases weakly with the strength of his/her preference for reciprocity. Note that the prediction is not only a back-transfer of zero or equal to the first mover's transfer, but that transfers in between are possible as well. However, the first mover's transfer is predicted to be either zero points or the whole endowment. He/she will transfer zero points if his/her second-order beliefs are low enough, that is, below a certain threshold. Otherwise, first movers will transfer the whole endowment. This threshold increases with the first mover's degree of reciprocity. Thus, the more reciprocal a first mover is, the less he/she is predicted to transfer in a trust game.

In sum, a preference for equity or reciprocity increases the likelihood that the second mover is behaving in a trustworthy manner. However, it decreases the probability that the first mover trusts given the second mover's preferences.

### 2.3.2 Selection Effects

Imagine the situation that someone knocks on your door and asks you to participate in a survey on politics and society that would last about an hour. Included in the survey is a 'game' in which you can earn some money. Many people will be suspicious and mistrust the person at the door. This situation is common for the interviewers in the social research section of TNS Infratest in Munich. They conduct the interviews for SOEP's accompanying studies as well as for the panel study itself. Besides other factors, the distrust in the interviewer and/or the survey organisation may lead people to refuse to participate in the survey.

If one wants to study trust on a representative level, it is therefore crucial to avoid a randomisation bias (Heckman and Smith, 1995) due to trust. The issue has grown in importance in light of recent criticism of experimental economics (Levitt and List, 2007), which is confronted with the same selection problem. So far, however, there has been little discussion about selection into either lab or field experiments. In one of the few studies that has addressed the problem, Bellemare and Kroeger (2007) do not find any randomisation bias for a trust experiment in a random sample of 541 regular panel participants in the Netherlands. Contrary, Harrison *et al.* (2009) find a randomisation bias in a sample of 253 Danish subjects who were not part of a panel study but recruited for a 'snapshot study' like our accompanying studies. They found that risk-averse people were less likely to participate in their study. These two studies indirectly assess the randomisation bias for the variable of interest.

There are two possible randomisation biases in our study design. The first is that less trusting persons will be less likely to participate in the survey and second, that

conditional on participation in the survey, it is less likely that they will agree to participate in the experiment at the end of the questionnaire. It is important to analyse these effects separately, since survey respondents know what the survey is about, while participants in an experiment normally do not see its ultimate purpose. We analyse the first randomisation bias in Appendix 2.C. In sum, we do not find any indication for selection due to trust into the survey.

The aim of this section is to examine the second randomisation bias, the selection into the experiment. This can be addressed directly by comparing trust measured in the survey for participants in the experiment with non-participants. Since all the subjects who refused to participate in the experiment completed the survey, we are able to directly assess their trust through trust measured in the survey. As a proxy for trust in the interviewer and the survey organisation, we take SOEP-trust and past trusting behaviour. The latter captures the experience of past interactions that involved trusting others. We find that in the SOEP, neither past trusting behaviour nor SOEP-trust are related to the refusal to participate in the experiment (Mann Whitney test: past trusting behaviour $z=0.673$ $p>0.500$; SOEP-trust 0.505 $p>0.613$). Contrary to the SOEP, we find in the accompanying studies a significant lower level of trust among people who refused to participate in the experiment than those who participated (Mann Whitney test: past trusting behaviour $z=2.23$ $p<0.026$; SOEP-trust 1.322 $p>0.185$).

One likely explanation for a randomisation bias due to trust in the AS but not in the panel study (SOEP) is based on the different set-ups. In the SOEP, subjects are familiar with the survey organisation and usually also with the interviewer, since these persons have been participants in the panel for three years. In accompanying studies, on the other hand, people are coming into contact with the survey organisation and the interviewer for the first time. Trust in strangers can thus only influence the participation decision in the AS and not in the SOEP since the interviewer is not stranger for the SOEP participants. According to this interpretation the refusal rates should be higher in the AS than in the SOEP. In the accompanying studies, we indeed find that in 2003, 5.1% and in 2004, 10.8% of subjects refused to participate in the experiment. In the panel study SOEP however, only 4.8% of 1,504 subjects who completed the questionnaire refused to participate in the experiment in 2003. The difference in refusal rates between AS and SOEP is highly significant (Fisher's exact test: $p<0.001$).

Furthermore, those people who refuse to participate in the experiment are also more likely to leave the panel (SOEP) in the following three years (Fisher's exact test: $p<0.001$). However, the people who left the panel did not exhibit different trust levels in the experiment (Mann-Whitney test: $z=0.387$ $p>0.698$). In the accompanying studies, we looked at people's willingness to participate in a similar study another time

compared to the decision to leave in the panel. As in the panel study, people who refuse to participate in the experiment are also less willing to participate another time in a similar study (Mann-Whitney test: z=10.641 p<0.000). Unlike in the panel study, we find that people who are less willing to participate in another similar study exhibit less trust in the experiment (Spearman's rank correlation $\rho = 0.08$ p<0.034).

Concerning risk-aversion we find very similar results as those for trust and found in Harrison *et al.* (2009). We measured people's risk aversion by asking 'Are you generally a person who is fully prepared to take risks, or do you try to avoid taking risks?'. People answered on a Likert scale. This measure of risk-aversion is shown to be a valid measure by comparison to a lottery experiment with real monetary stakes (Dohmen *et al.*, forthcoming). As with trust, we find that in the panel study SOEP, there is no significant relation between risk preferences and the participation rate (Mann-Whitney test: z=1.487 p>0.136), whereas in the AS03 the most risk-averse people are less likely to participate in the experiment than the least risk-averse (Mann-Whitney test: z=2.135 p<0.033).

Thus, our results suggest that representative trust games are subject to selection effects due to trust and risk preferences if subjects are unfamiliar with the situation they are in (e.g., unknown survey organisation and/or interviewer).

In Appendix 2.C, we additionally analyse other factors that might influence the participation in the experiment, including social preferences, personality, and demographic variables. In sum we find that selflessness, reciprocity, and interviewer characteristics do not matter in the decision to participate in the experiment. A medium income, a large household size, and living in Eastern Germany reduce the probability that a person will refuse to participate. Finally, the longer the previous questionnaire lasted, the more likely it is that participants will refuse to participate in the present experiment in the AS.

In sum, we found that the level of trust is related to the decision to participate in the experiment for subjects who are participating for the first time in this kind of interview. On the other hand, trust has no influence if subjects are familiar with the general set-up of the study. Based on these findings we conclude that a longitudinal panel survey where a 'trust relationship' between the survey institute and the respondents is already established is the best way to minimise the total response error when adding experimental add-ons.

### 2.3.3 Validity of Experimental Measure

The decision to trust is influenced by people's preferences and expectations. Risk preferences and expectations about the other player's behaviour are expected to shape the

decision to trust (Coleman, 1990). Thus, one can check the validity of the trust measure by showing that these preferences and expectations indeed correlate with EXP-trust.

We measured people's risk-aversion as outlined in section 2.3.2. We find that the first mover transfer is higher the less risk-averse people are in AS03 (Spearman's rank correlation $\rho = 0.13$ $p<0.012$). Thus, people's risk preferences influence their decisions to trust or mistrust.

In the AS03 we elicited first movers' expectations by asking how much they expected the second mover to transfer back if they were to transfer zero, five, or ten points to the second mover. A selfish first mover would like to maximise his/her payoff. We therefore calculated which of the three transfers (zero, five, or ten) a first mover expected to maximise his/her payoff. The higher the transfer is that maximises expected profits, the more a selfish first mover is expected to transfer. We find a positive and significant correlation (Spearman's rank correlation $\rho = 0.20$ $p<0.003$). However, a non-selfish first mover is not primarily interested in making the transfer that he/she expects to maximise profits. If he/she cares about inequality, he/she would like to know which of the three transfers yields the lowest inequality. With our data, this is also possible to calculate, and the higher the transfer expected to minimise inequality is, the higher we expect the first movers' transfers to be. This other measure for expectations is positively correlated with first mover's transfer, as well (Spearman's rank correlation $\rho = 0.31$ $p<0.001$). As a third measure, we calculate the average expected back-transfer, which can be interpreted as a general measure for the expectation of the second mover's selflessness. Again, we find a positive correlation (Spearman's rank correlation is: $\rho = 0.18$ $p<0.005$).

In sum, we have shown that EXP-trust is influenced by risk preferences and by expectations about the second mover's behaviour. Further, in Section 2.4, we demonstrate that EXP-trust is correlated with survey measures of trust. All these results are strong indications that the EXP-trust is a valid measure of trust.

### 2.3.4 Sensitivity of the Experimental Measure

In this section we analyse whether students, as a subset of the general population, behave differently; whether the size of the stakes in the trust game matters and whether trusting behaviour is influenced by social desirability. In Appendix 2.D we further analyse the impact of the use of the strategy method and the number of possible choices a first-mover has.

**Students versus General Population**

A common critique of laboratory experiments is that students, the preferred subject pool in experiments, may behave systematically differently than non-students (Levitt and List, 2007). In addition, there are a number of studies showing that students behave differently than other groups or that economics students are different from non-economics students (e.g. Fehr *et al.*, 2006). A stronger test of the claim that students behave systematically differently is to compare their behaviour to a representative sample of the general population. Only a few studies have been designed this way, and the following results are found. In the U.S. state of Vermont, Carpenter *et al.* (2008) found in a field experiment that students donated 17 dollars less to charities than non-students, who donated 72 dollars out of 100. In a representative ultimatum game in Taiwan, no difference was found between students and non-students (Fu *et al.*, 2007). Concerning discount rates, Harrison *et al.* (2002) found that in Denmark, students have a six percentage point higher discount rate than non-students. In a similar study in Denmark, students were found to be more risk-averse than non-students (Harrison *et al.*, 2007). Since students are found to be more risk-averse and less pro-social, previous studies have suggested that students transfer fewer points in trust games than non-students. Indeed, this result was found by Bellemare and Kroeger (2007) for the Netherlands, where students transferred much less in a trust experiment than a representative population sample.

Among our German sample of 1,665 first movers in the trust game, we identify 47 as students. We find the opposite of all previous studies in that students transfer 61% whereas non-students transfer 50% of their endowment. Thus, students exhibit a 21% higher level of trust than non-students. This is difference is highly significant (regression 1 in Table 2.4). The results from the survey measure of trust strongly support these findings. SOEP-trust, trust in institutions, and past trusting behaviour are more pronounced among students (Mann-Whitney test: SOEP-trust $z = 10.521$ $p<0.001$; trust in institutions $z = 2.88$ $p<0.005$; trust in known others $z = 0.01$ $p>0.99$; past trusting behaviour $z = 19.78$ $p<0.001$). Students are typically younger than the average population, have a lower income, and a higher level of education. Do these different characteristics explain the observed differences? When controlling for age and income, we find that these variables cannot explain the higher level of trust in students. The coefficient hardly changes, although the significance level slightly decreases (regression 2 in Table 2.4). Furthermore, in the German population with our survey measure of risk aversion, we find that students seem to be less risk-averse than non-students. Therefore it is important to control for whether lower risk aversion can explain the higher level of trust among students than non-students. As expected, risk aversion is a determinant of trusting behaviour but the differences between students and non-students remains

weakly significant (regression 3 in Table 2.4). However, controlling for education reduces the student dummy substantially and renders the coefficient insignificant (regression 4 in Table 2.4). Having a higher education is related to high trust levels which explains the higher level of trust among students compared to non-students.

Table 2.4: Dependent variable: level of trust (first mover transfer)

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Dummy for being a student | 0.897$^{\star}$ | 0.836$^{\circ}$ | 0.981$^{\circ}$ | 0.535 |
| | (0.436) | (0.445) | (0.591) | (0.468) |
| Age | | −0.003 | −0.002 | −0.001 |
| | | (0.004) | (0.005) | (0.004) |
| Household income: <2500 Euro | | −0.050 | 0.006 | −0.060 |
| (Base: <1500 Euro) | | (0.195) | (0.239) | (0.195) |
| Household income: <3500 Euro | | −0.102 | −0.240 | −0.167 |
| | | (0.212) | (0.258) | (0.213) |
| Household income: <5000 Euro | | 0.541$^{\star}$ | 0.648$^{\star}$ | 0.434$^{\circ}$ |
| | | (0.238) | (0.285) | (0.240) |
| Household income: >5000 Euro | | 0.987$^{\star\star}$ | 1.071$^{\star\star}$ | 0.825$^{\star\star}$ |
| | | (0.301) | (0.374) | (0.310) |
| Risk aversion: medium | | | 0.224 | |
| (Base: high) | | | (0.187) | |
| Risk aversion: low | | | 0.529$^{\star}$ | |
| | | | (0.252) | |
| Intermediate secondary school | | | | 0.165 |
| (Base: less than interm. secon. school) | | | | (0.169) |
| High school and more | | | | 0.522$^{\star\star}$ |
| | | | | (0.198) |
| Constant | 5.080$^{\star\star}$ | 5.118$^{\star\star}$ | 4.907$^{\star\star}$ | 4.932$^{\star\star}$ |
| | (0.071) | (0.273) | (0.375) | (0.290) |
| N | 1550 | 1550 | 1010 | 1550 |
| Adjusted $R^2$ | 0.002 | 0.013 | 0.024 | 0.016 |

*Notes.* OLS regression, coefficients and robust standard errors (in parenthesis) reported. The number of observations is lower in model 3 since data on the survey question on risk aversion is only available for people in the SOEP and AS03. $^{\circ}$, $^{\star}$, $^{\star\star}$ denote statistical significance on the 10%, 5% and 1% level, respectively.
*Sources.* SOEP 2003 and AS02, AS03, and AS04

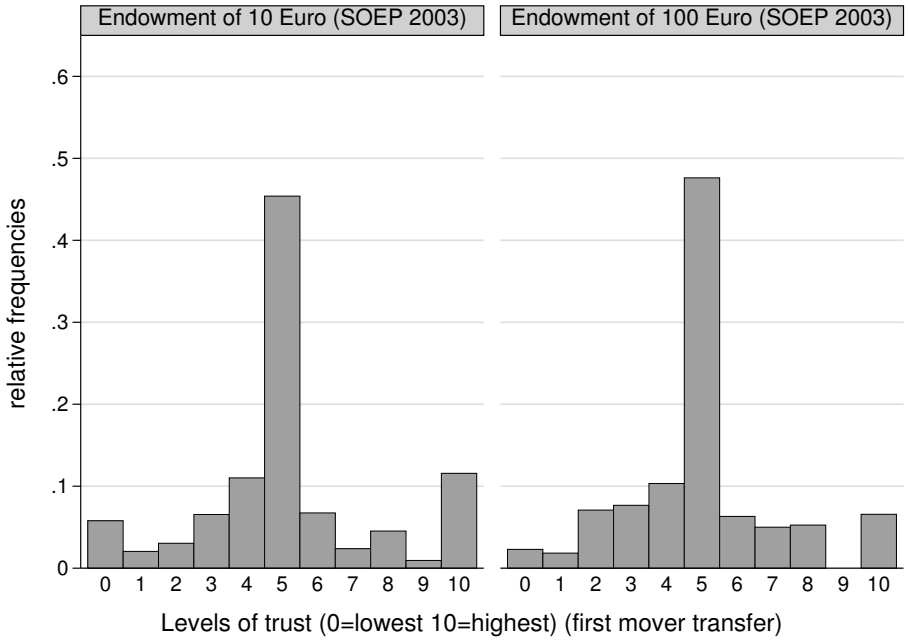
Trustworthiness does not differ between students and non-students (regression 1 in Table 2.5). Thus, students exhibit a higher level of trust than non-students. The difference is not due to a different age, income or differences in risk preferences. Education is the driving force behind the higher level of trust among students. People with a higher education trust more.

### Stakes

Only a few studies have examined the effects of stake size on behaviour in experiments. Most of these studies have analysed the ultimatum game and found that first movers' offers are independent of the stake size[7]. However, it is also found that the respondents' minimal acceptable offer is lower with high stakes than with low stakes (Cameron, 1999; Fu *et al.*, 2007; Hoffman *et al.*, 1996; Munier and Zaharia, 2002; Slonim and Roth, 1998). No significant effects are found in the dictator game (Carpenter *et al.*, 2005; Cherry *et al.*, 2002; Forsythe *et al.*, 1994; List and Cherry, 2008) or in the gift-exchange game (Fehr *et al.*, 2002a). Mutual trust measured in the centipede game is reduced significantly with lower stakes (Parco *et al.*, 2002).

Johansson-Stenman *et al.* (2005a) are the only ones that have studied the effect of stake size in the trust game. In contrast to the findings in the gift-exchange game and the ultimatum game and in line with the centipede game, they found that first movers' transfers in rural Bangladesh were lower the higher the stakes. The proportions transferred to the second movers was 55% in the case with low stakes. The proportion transferred decreased to 46% in the medium-stakes condition, and to 38% in the high-stakes condition. The stakes were equivalent to 67, 337 and 1683 U.S. dollars. Concerning the behaviour of the second-mover, no difference was found for the different stake sizes.

Figure 2.1: Levels of trust: different stake sizes




[7]An exception was reported by Fu *et al.* (2007), who found that offers decrease with stakes.

We compared the differences between endowments of 10 and 100 euros. Contrary to the results of Johansson-Stenman *et al.* (2005a), we find no differences in the average level of trust (Figure 2.1: t-test: t = 0.50 p>0.62). The high-stakes endowment was ten times higher than the low-stakes one, and so was the average transfer (5.16 versus 49.9 euros). The distributions also do not differ (Kolmogorov-Smirnov test: p>0.968). The levels of trustworthiness are not different in the two treatments either (regression 2 in Table 2.5).

### Social Desirability

Contrary to the questionnaire, the decision in the experiment remains private and is not communicated to the interviewer. Thus social desirability is not expected to influence the decision. We again use the Balanced Inventory of Desirable Responding by Paulhus (1991). We indeed find that impression management (Spearman's rank correlation $\rho =$ 0.08 p>0.145) and self-deception (Spearman's rank correlation $\rho = 0.03$ p>0.590) are not significantly correlated with EXP-trust. The decision to behave trustworthily or not is not correlated with social desirability either (regression 3 in Table 2.5).

Table 2.5: Sensitivity of experimental measure of trustworthiness

| Dependant variable: dummy of being trustworthy | Students | Stakes | Social desirability |
|---|---|---|---|
| Dummy for being a student | −0.001 | | |
| | (0.048) | | |
| Dummy for 100 Euro treatment | | −0.072 | |
| | | (0.052) | |
| Soc. desir.: Impression (std.) | | | −0.020 |
| | | | (0.028) |
| Soc. desir.: self-deception (std.) | | | −0.001 |
| | | | (0.028) |
| Constant | 0.535** | 0.518** | 0.606** |
| | (0.020) | (0.047) | (0.058) |
| Controlled for first mover transfer | Yes | Yes | Yes |
| N | 1912 | 602 | 292 |
| Adjusted $R^2$ | 0.131 | 0.166 | 0.014 |
| Cluster on individual level | Yes | No | No |

*Notes.* OLS regression, coefficients and robust standard errors (in parenthesis) reported. °, ⋆, ⋆⋆ denote statistical significance on the 10%, 5% and 1% level, respectively.
*Sources.* AS02, AS03, AS04 and SOEP 2003

## 2.4 Comparing Experimental and Survey Measures of Trust

It yet remains uncertain if our experimental measure captures the same kind of trust as is described by the various survey measures of trust. Previous research yielded ambiguous answers when comparing EXP-trust to GSS-trust. Given the criticisms lodged against this measure, this may not come as a surprise. In GSS-trust, it is unclear, for example, what or who it is that we trust. In Section 2.2.3 we showed that GSS-trust is correlated with several dimensions of trust such as trust in strangers, trust in known others, and trust in institutions. Unclear relations between GSS-trust and the experimental measure of trust are probably due to weakness of the survey question rather than problems with the experiment. Further indications for this can be found in Glaeser *et al.* (2000) and Gächter *et al.* (2004). Both studies report that their experimental measure of trust is not correlated with GSS-trust.

However, Glaeser *et al.* (2000), Gächter *et al.* (2004), and Csukas *et al.* (2008) find that their experimental measures of trust are clearly correlated with survey questions on past trusting behaviour and a question on trust in strangers, which is formulated as the statement 'You can't count on strangers anymore' and is similar to statements on which SOEP-trust is built. Thus, the question should not be whether survey measures of trust are correlated with experimental measures or not, but rather what kind of trust the trust game measures. To this effect, we measured trust in strangers (SOEP-trust), trust in known others, trust in institutions, and past trusting behaviour.

With our surveys, we have all the ingredients needed to test what kind of trust the trust game (EXP-trust) actually measures. We find that EXP-trust is significantly correlated ($\rho = 0.12$) with SOEP-trust (Table 2.6). Not only the overall measure (SOEP-trust) but also all its components are correlated with the experimental measure. Furthermore, EXP-trust is not correlated with the index 'trust in institutions' nor the index 'trust in known others'. The fact that none of the single items of these two indexes are correlated significantly with EXP-trust further confirms that the experimental measure specifically measures trust in strangers.

Furthermore, we find that self-reported past trusting behaviour is significantly correlated with the experimental measure of trust ($\rho = 0.16$) and that all the three items of the index 'past trusting behaviour', – lending money, lending possessions, and leaving the door unlocked – are significantly related to experimental trust.

We additionally analysed two frequently used survey questions on fairness and helpfulness that are implemented in the GSS as well. The questions are *'Do you think most people would try to take advantage of you if they got a chance, or would they try to be*

Table 2.6: Correlations of different survey measures of trust with EXP-trust

| | Spearman's $\rho$ | sign.-level | obs. |
|---|---|---|---|
| **Different indices of survey measures of trust** | | | |
| SOEP-trust | 0.116 | 0.000 | 1661 |
| Trust in institutions | 0.022 | 0.493 | 952 |
| Trust in known others | 0.013 | 0.682 | 949 |
| past trusting behaviour | 0.156 | 0.000 | 1654 |
| | | | |
| **To what extent do you agree or disagree?** | | | |
| In general you can trust people. | 0.066 | 0.007 | 1660 |
| Nowadays, you can't rely on anybody. | −0.107 | 0.000 | 1702 |
| It's better to be cautious before trusting strangers. | −0.099 | 0.000 | 1704 |
| **How much trust do you have in...** | | | |
| strangers | 0.137 | 0.000 | 732 |
| your own family | −0.008 | 0.814 | 946 |
| neighbours | 0.010 | 0.768 | 947 |
| friends | 0.038 | 0.239 | 946 |
| co-worker | 0.041 | 0.257 | 764 |
| churches | 0.001 | 0.965 | 940 |
| schools and the educational system | 0.054 | 0.102 | 930 |
| press | −0.014 | 0.669 | 945 |
| labour unions | −0.013 | 0.688 | 923 |
| police | 0.027 | 0.411 | 948 |
| parliament | 0.034 | 0.292 | 943 |
| public authorities | 0.025 | 0.439 | 945 |
| the European Union | 0.018 | 0.591 | 923 |
| courts | 0.037 | 0.259 | 941 |
| large companies | −0.034 | 0.301 | 931 |
| **How often do you ...** | | | |
| lend personal possessions to friends? | 0.140 | 0.000 | 1653 |
| lend money to your friends? | 0.097 | 0.000 | 1653 |
| leave your door unlocked? | 0.107 | 0.000 | 1692 |
| Do you think most people would try to take advantage of you if they got a chance, or would they try to be fair? (GSS-fair) | 0.067 | 0.006 | 1679 |
| Would you say that most of the time people try to be helpful, or that they are mostly just looking out for themselves (GSS-help) | −0.004 | 0.880 | 1683 |

*Sources.* SOEP 2003 and AS02, AS03, and AS04

*fair?'* (GSS-fair) and *'Would you say that most of the time people try to be helpful, or that they are mostly just looking out for themselves'* (GSS-help). We find that trusting behaviour is significantly positively correlated with fairness measured by GSS-fair but that the correlation is rather low ($\rho = 0.07$). GSS-help, on the other hand, is not related to trusting behaviour.

We have thus shown that trust measured in the experiment actually measures a specific kind of trust, namely trust in strangers. Whereas this in itself is perhaps not surprising, given the nature of the experiment where participants are by design unknown strangers to each other, it is noteworthy on the other hand that EXP-trust has almost nothing to do with trust in institutions nor with trust in known others.

The correlations of trust measured in the experiment and SOEP-trust ($\rho = 0.12$) and past trusting behaviour ($\rho = 0.16$) are rather low, although they are significantly different from zero. The reason for this could be that the survey measure SOEP-trust mainly measures expectations of other people's trustworthiness. The statements 'In general, you can trust people' and 'Nowadays, you can't rely on anybody' give strong indications of how a person sees strangers. In the experiment, however, the decision to trust or not is not only influenced by expectations of others' trustworthiness but also by risk and social preferences (chapter 3). If the measure SOEP-trust in fact mainly measures the expectation part of the motivation to trust, then we would expect the correlations between the survey measure and expectations in the experiment to be rather high – in particular, higher than the correlations with the actual decision (for a related argument see also Sapienza *et al.* (2007)).

Table 2.7: Correlations between expectations and behaviour in the experiment

| | **Exp. back-transfer at a transfer of ...** | | | **EXP-trust** | |
|---|---|---|---|---|---|
| | **0 points** | **5 points** | **10 points** | **uncontrolled** | **controlled for expectations** |
| SOEP-trust | 0.11** | 0.25** | 0.16** | 0.10* | 0.05 |
| Trust in known others | 0.02 | 0.13* | 0.10* | 0.03 | -0.02 |
| Trust in institutions | 0.03 | 0.17** | 0.04 | -0.00 | -0.00 |
| past trusting behaviour | -0.05 | 0.20** | 0.15** | 0.11* | 0.07° |
| (number of observations) | (556) | (296) | (556) | (556) | (556) |

*Notes.* Pearsson's correlations. The table reports the correlation coefficients. °, *, ** denote statistical significance on the 10%, 5% and 1% level, respectively.
*Sources.* AS03 and AS04

We measured first movers' expectations of the behaviour of the second movers as outlined in Section 2.3.3. We indeed find that survey measures of trust correlate more strongly with expectations than with first mover transfers (Table 2.7). The correlation

between SOEP-trust and EXP-trust is mainly driven by the correlation between SOEP-trust and expectations. This can be seen by looking the partial correlation of SOEP-trust with EXP-trust while controlling for the expectations at transfers of 0 and 10 points.[8] The partial correlation ($\rho = 0.05$) is half the full correlation ($\rho = 0.10$) and not significant whereas the uncontrolled correlation is significant. From this we conclude that SOEP-trust measures the expectations part of the decision to trust.

Note that expectations correlate not only with SOEP-trust but also with our measures for trust in institutions and trust in known others, too. Thus, expectations seem to be less specific to one dimension of trust than is the first mover decision in the trust game. In sum, whereas expectations measure a somewhat unspecified kind of trust, the decision in the experiment specifically measures trust in strangers.

Table 2.8: Different trust measures and recently found correlations of trust

| | **EXP trust** | **SOEP trust** | **SOEP trust** | **Exp. back-transfer at a transfer of ...** | | |
|---|---|---|---|---|---|---|
| | **(same obs.)** | | **(all obs.)** | **0 points** | **5 points** | **10 points** |
| Age | -0.04° | -0.01 | -0.01° | 0.05° | -0.05 | -0.10** |
| Being female | -0.00 | 0.01 | -0.02** | 0.01 | -0.02 | -0.08** |
| Education | 0.09** | 0.11** | 0.16** | -0.06* | 0.11** | 0.12** |
| Household income | 0.08** | 0.14** | 0.15** | -0.04 | 0.03 | 0.10** |
| Being foreigner | -0.05° | -0.02 | -0.04** | 0.03 | -0.02 | -0.03 |
| Living in East Germany | -0.06* | -0.11** | -0.09** | -0.01 | 0.05 | -0.06* |
| Religious | 0.00 | 0.12** | 0.11** | -0.01 | 0.06° | 0.10** |
| Being undenominational | 0.01 | -0.08** | -0.06** | -0.02 | 0.04 | -0.02 |
| Risk aversion | -0.09** | -0.08* | -0.15** | -0.00 | 0.02 | -0.07° |
| Negative reciprocity | -0.06° | -0.08* | -0.10** | -0.03 | 0.01 | 0.01 |
| Freq. of volunteering | 0.06* | 0.12** | 0.13** | 0.02 | 0.01 | 0.08** |
| Being an entrepreneur | 0.06° | 0.04 | 0.04** | -0.00 | 0.00 | -0.00 |
| Being a shareholder | 0.07° | 0.05 | 0.12** | -0.07° | 0.02 | 0.12** |
| Appr. number of obs. | 1,660 | 1,660 | 25,500 | 1,150 | 860 | 1,150 |

*Notes.* Spearman's rank correlations. The table reports the correlation coefficients. °, *, ** denote statistical significance on the 10%, 5% and 1% level, respectively.
*Sources.* SOEP 2003, SOEP 2005, AS02, AS03, AS04, and AS05

Trust is a widely researched concept and many different factors have been found to be correlated with trust. Therefore, we address the question if these factors are correlated with the experimental measure of trust similarly – or differently – than how they are correlated with survey measures of trust. As a further illumination of the relation of survey and experimental measures of trust we investigate exactly the level of similarity. In order

[8] We did not ask the expectation for a transfer of 5 in the AS04.

to answer this question, we chose prominent factors that previous research has shown to be correlated with trust. As measures of trust, we compare trusting behaviour in the experiment, SOEP-trust, and the different measures of expectations in the experiment as explained above. Several studies have reported that socio-economic variables such as age, gender, income, education, nationality, and place of living are correlated with trust (e.g. Alesina and La Ferrara, 2002; Bellemare and Kroeger, 2007; Buchan *et al.*, 2008; Rainer and Siedler, 2009; Sutter and Kocher, 2007). We find that the behavioural trust measure and SOEP-trust are correlated in similar ways with socio-economic variables[9] (Table 2.8). The only exception is gender where we find lower trust in women than in men when measured by survey, but no gender difference in trust when measured by experiment. Concerning religion, we find that people with no religious affiliation exhibit lower SOEP-trust and people who are actively religious have higher SOEP-trust. These results are similar to those found in Guiso *et al.* (2003) with survey measures of trust. However, we cannot confirm these two effects with the behavioural measure of trust for Germany. Risk and social preferences (volunteering and negative reciprocity) are correlated significantly with both the survey and the experimental measure of trust with similar magnitudes. Finally, trust is found to be higher among entrepreneurs and shareholders (Guiso *et al.*, 2006, 2008). In our data, we find that both EXP-trust and SOEP-trust are higher for entrepreneurs and shareholders. In sum, we find that trust measured by the experiment has similar correlations with factors that have been reported, as trust measured by survey. This is a further indication that both - EXP-trust as well as SOEP-trust - are valid measures of trust.

Another interesting finding is that SOEP-trust is related to preference measures such as risk and social preferences. This points to the fact that SOEP-trust does not only measure the expectation part of the decision to trust but also the influence of preferences. That SOEP-trust not exclusively measures expectations is further supported by the fact that correlations of expectations with these various variables yield a different, mixed picture. The expectation at a transfer of 10 points yields similar results as SOEP- and EXP-trust. The expectations at a transfer of 0 and 5 points, however, yield opposite results for age and education and no significant correlation with many other variables.

Overall, we find that SOEP-trust and EXP-trust are significantly correlated. A large part of the correlation between SOEP- and EXP-trust can be explained by the relatively high correlation between SOEP-trust and expectations. SOEP- and EXP-trust correlate in a similar way with various other variables (e.g. social and risk preferences and being and entrepreneur and shareholder), which suggests that both measures are valid measures of trust.

[9]We do not find that the relation of trust and age is quadratic

## 2.5 Conclusions

We have developed a compact survey measure of trust in strangers (SOEP-trust) that takes into account recent criticism of the widely used GSS and WVS question. We rigourously tested this measure and find that it is a valid and reliable measure of trust in strangers.

We analysed an experimental measure of trust extensively and most importantly, always in the same setting. We showed that there may be a selection of more trusting people into the experiment if the individuals are participating in such a survey for the first time, whereas in the panel study, we do not find that selection is an issue. Moreover, the experiment is quite insensitive to various changes. We find that stakes, social desirability, strategy space, and use of the strategy method do not affect the behaviour in the experiment in significant ways. However, we find that students, who are typical subjects of lab experiments, behave differently than non-students in that they trust strangers more than non-students. This finding is confirmed by the survey, where we find that SOEP-trust is higher among students than among non-students. Furthermore, we show that trusting behaviour is influenced by people's risk and social preferences as well as their expectations.

In combination, we find that the experimental measure of trust is significantly correlated with SOEP-trust, which is specifically aimed to measure trust in strangers; but not with an index of trust in institutions and an index of trust in known others. Furthermore, experimental trust correlates with related factors similarly as SOEP-trust does. We conclude that the common experimental measure of trust - the so-called 'trust game' - is a valid measure for empirical research, which captures a specific form of trust: trust in strangers.

## Acknowledgments

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# 2.A Data Overview

Table A.1: Data overview

| Study | No. of obs. | | stake | scale | strategy | survey |
|---|---|---|---|---|---|---|
| | Survey | Exp. | size | | method | sensitivity |
| AS02 | 442 | 442 | 10 | 11 options | no | |
| SOEP 03 | 22'611 | 1'432 | 10/100 | 11 options | no | |
| AS03 | 846 | 803 | 10 | 3 options | yes | |
| AS04 | 772 | 689 | 10 | 6 options | no | NEO-FFI |
| AS05 | 1'012 | | | | | reliability |
| AS06 | 1'063 | | | | | question order |
| | | | | | | ESS question |

All questionnaires can be downloaded from http://www.diw.de/soep.

# 2.B Selection Effects: Survey

Did only highly trusting people participate in the survey? If this were the case, we could not claim that our results are representative for Germany. We address this potential problem in three different ways. First we compare the level of trust when people are weighted such that the distribution in some basic characteristics conforms with the population distribution and when people are not weighted. Second, we check whether it makes a difference whether people are familiar with the survey organisation and/or the interviewer or whether the situation is unfamiliar by comparing the AS03 with the SOEP in 2003. Finally we know who has left the panel since 2003 and we can check whether this decision is dependent on their trust level in 2003.

In the accompanying studies, the unweighted mean of SOEP-trust (2.38) lies in the 95% confidence interval of the weighted mean (2.35 - 2.39). Similarly, no differences between the weighted and unweighted means are found for 'trust in known others' or for 'trust in institutions'. In the SOEP, the unweighted mean of SOEP-trust (2.308) lies inside the 95% confidence interval of its weighted mean (2.29 - 2.31) as well. Further the weighted means in the AS03 (2.30) and in the SOEP 2003 (2.30) are not significantly different from each other (t-test: $t = 0.03$, $p > 0.974$). That is, it makes no differences for SOEP-trust whether it is the first time people are interviewed, as in the accompanying study, or whether people have been in a panel for at least four years. The final test for a randomisation bias is that we know who left the panel between 2004 and 2006 and we know what their level of trust was in 2003. There is no difference in the mean of SOEP-trust between those who left the panel and those who stayed the following

two years (t-test = 1.24 P > 0.21). In the accompanying studies we do not have this measure, but people were asked whether they would like to participate in similar study again. Again this can be used as an indication whether there is a randomisation bias due to trust. Again, we do not find any significant difference (t-test = 1.34 P > 0.17).

In sum we find that the participation in the survey in general is not influenced by how much trust people have.

## 2.C Selection Effects: Experiment

Beside trust we are able to test whether social preferences, personality characteristics, demographic variables, interviewer characteristics and the length of the questionnaire are determinants of refusal to participate in the experiment (Table C.1). As a proxy for social preferences we take the frequency with which subjects volunteer and participate in politics and citizens' initiatives. Again, we find that in the AS and in the SOEP 2003 there is no significant impact on the rate of refusal. Concerning the personality measures we find that positive and negative reciprocity do not predict refusal in the experiment either. However, some demographic variables do explain refusal to participate. There is a slight tendency for married people to be less likely to participate in the experiment. Further we find that in the SOEP 2003, people with a high or low household income are more likely to refuse to participate. In the AS, people in larger households are more cooperative in participating, whereas East Germans are less likely to participate. A further test is the length of the survey as a proxy for an additional response burden. The interviews in the AS04 were conducted using a laptop that recorded the time used for each question. We thus test whether the length of time from the beginning of the survey to the decision to participate in the experiment predicts this decision. We indeed find that the longer the questionnaire, the less likely it is that participants agree to participate in the subsequent experiment in AS04.

Furthermore, the interviewer is a possible source of influence on participation in the experiment. However, we do not find interviewer characteristics such as age, gender and years of experience to be influential (models 1 - 3 in Table C.2).

In sum we find that selflessness, reciprocity, and interviewer characteristics do not matter in the decision to participate in the experiment. A medium income, a large household, and living in Eastern Germany reduce the probability that a persons will refuse to participate. Finally the longer the previous questionnaire lasted, the more likely it is that participants will refuse to participate in the present experiment in the AS.

Table C.1: Selection into the experiment

| Dep. var.: Dummy of whether a person refused to participate in the experiment | SOEP03 | AS03 | AS04 |
|---|---|---|---|
| Dummy of volunteering at least sometimes | −0.008 | 0.008 | 0.044 |
| | (0.013) | (0.014) | (0.028) |
| Dummy of participating in political parties, | −0.005 | 0.039 | −0.010 |
| citizens' initiative | (0.017) | (0.029) | (0.027) |
| Dummy of being female | 0.001 | 0.016 | −0.028 |
| | (0.011) | (0.010) | (0.022) |
| Age | 0.000 | 0.000 | 0.000 |
| | (0.000) | (0.000) | (0.001) |
| Intermediate secondary school | 0.015 | −0.012 | −0.007 |
| (Base: less than interm. secon. school) | (0.015) | (0.011) | (0.025) |
| High school and more | −0.007 | 0.004 | 0.017 |
| | (0.014) | (0.015) | (0.035) |
| Living with partner (Base: Married) | 0.010 | −0.010 | −0.049° |
| | (0.025) | (0.013) | (0.028) |
| Single or not living with partner | −0.009 | −0.024° | −0.022 |
| | (0.013) | (0.012) | (0.027) |
| Household income: <2500 Euro | −0.034** | 0.003 | −0.019 |
| (Base: <1500) | (0.012) | (0.014) | (0.028) |
| Household income: <3500 Euro | −0.028* | 0.015 | −0.033 |
| | (0.013) | (0.022) | (0.029) |
| Household income: <5000 Euro | −0.019 | 0.012 | −0.042 |
| | (0.015) | (0.027) | (0.032) |
| Household income: >5000 Euro | −0.009 | 0.008 | −0.003 |
| | (0.020) | (0.030) | (0.055) |
| Household size | 0.007 | −0.026** | −0.011 |
| | (0.005) | (0.007) | (0.012) |
| Dummy of living in east Germany | −0.011 | −0.012 | −0.053* |
| | (0.012) | (0.011) | (0.021) |
| Dummy of being foreigner | −0.013 | | 0.005 |
| | (0.021) | | (0.061) |
| Negative reciprocity | | | 0.009 |
| | | | (0.007) |
| Positive reciprocity | | | 0.015 |
| | | | (0.011) |
| Length of interview (in minutes) | | | 0.002** |
| | | | (0.001) |
| Pr(refusal) | 0.045 | 0.022 | 0.081 |
| N | 1480 | 716 | 659 |
| log-likelihood | −278.269 | −107.316 | −193.788 |
| Prob $> \chi^2_{crit.}$ | 0.581 | 0.014 | 0.062 |

*Notes.* Probit regression, marginal effects and standard errors (in parentheses) reported. °, *, ** denote statistical significance on the 10%, 5% and 1% level, respectively.

*Sources.* SOEP 2003, AS03, and AS04

Table C.2: Selection and interviewer characteristics

| Dep. var.: Dummy of whether a person refused to participate in the experiment | SOEP03 | AS03 | AS04 |
|---|---|---|---|
| Years of experience in polling firm | 0.001 | −0.000 | −0.002 |
| | (0.001) | (0.001) | (0.003) |
| Dummy for a female interviewer | 0.022 | −0.024 | −0.040 |
| | (0.014) | (0.015) | (0.044) |
| Age of the interviewer | −0.000 | −0.001 | −0.002 |
| | (0.001) | (0.001) | (0.002) |
| N | 1491 | 729 | 558 |
| Adjusted $R^2$ | 0.005 | −0.001 | 0.005 |
| Prob $> F_{crit.}$ | 0.116 | 0.229 | 0.559 |

*Notes.* OLS regression, coefficients and robust standard errors (in parentheses) reported. °, ⋆, ⋆⋆ denote statistical significance on the 10%, 5% and 1% level, respectively.
*Sources.* SOEP 2003, AS03, and AS04

# 2.D Sensitivity of the Experimental Design

## 2.D.1 Design

In the AS02 and the SOEP 2003 we observed that the modus of the distribution of first movers' choices is to transfer half of the endowment (36% and 45%). This result is not an artefact of our specific study design or our instructions since in other studies half of the endowment is the modal choice as well (e.g. Bellemare and Kroeger, 2007; Berg *et al.*, 1995). The question arises whether the widely observed pattern is dependent on the design of the trust game. More specifically we ask how the level of trust changes if we reduce the number of choices and remove the possibility to transfer half of the endowment. To test this, we run three different experiments. In the SOEP 2003, we have run the basic experiment with 11 options for a transfer, which run from 0 to 10 points. In the AS03 we reduced the choices to only three transfer options, which were 0, 5, or 10 points. In the AS04 we eliminated the choice for a transfer of 5 points by allowing only transfer levels of even numbers.

The distributions of transfers in the three experiments are very different by construction (Figure D.1). However, the average transfer was almost the same in the three experiments. In the SOEP 2003 with 11 options 51.5% of the endowments was transferred, in the AS03 51.3% and in the AS04 50.3% of the endowment was transferred. These differences are far from being significant.[10] The probability of behaving trustwor-

[10]Two-sided t-test for differences in the mean: SOEP 03 vs. AS03 t = 0.46 p>0.64; SOEP 03 vs AS04 t = 0.12 p>0.90; AS03 vs. AS04 t = 0.32 p>0.74

thy and second-movers' back-transfers are not dependent on how many options subjects have either (Figure D.2 and regression 1 and 3 in Table D.1).

Figure D.1: Levels of trust: different scales

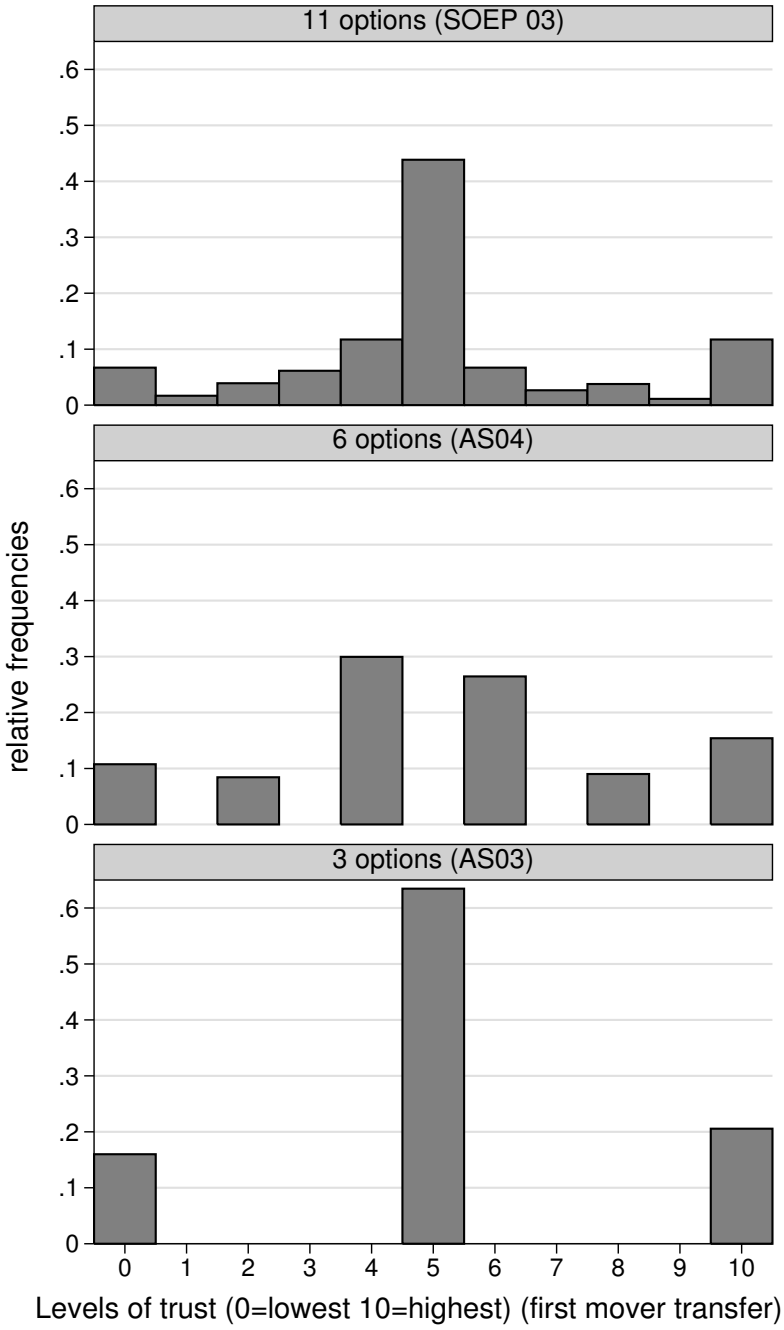
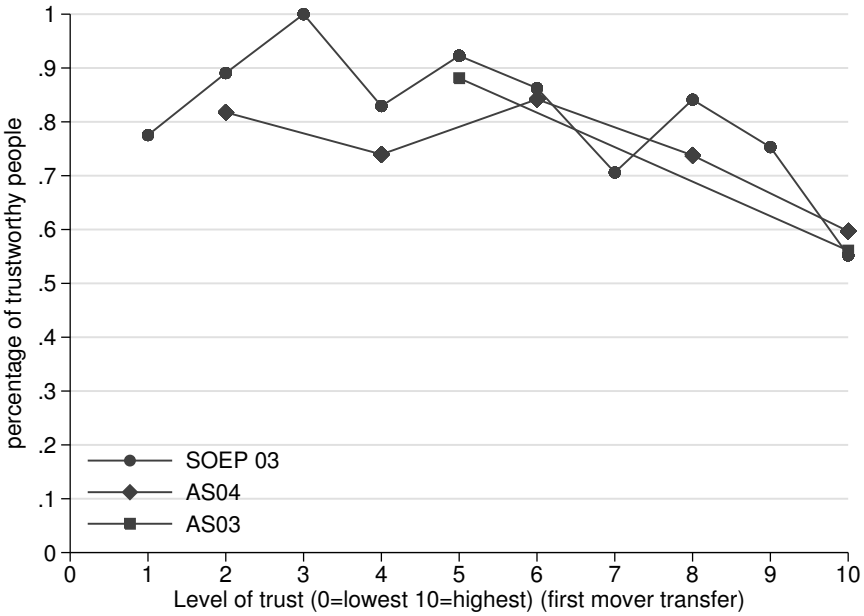


Figure D.2: Levels of trustworthiness




## 2.D.2 Strategy Method

The strategy method is a widely used elicitation procedure in experimental economics. With this method, second movers are asked to decide on a reaction to every possible first mover decision, but payoff occurs only for the one first-mover decision which was actually taken. In the trust game, this procedure allows us to distinguish between selfish players and conditionally cooperative players as well as between the latter and altruistic players. If a second mover in a direct method set-up, for example, receives zero points from the first mover and he/she does not transfer anything back, we gain no information on whether he/she would transfer a positive amount back if a first mover had transferred 5 points. The disadvantage is clearly that it is more complicated to explain to subjects, and the incentives are diluted since only one decision will actually be paid out. A further potential disadvantage is that the conditional decisions are less emotionally arousing than in the situation in which one knows what the other person decided. This might have the effect of gaining overall more rational responses and obscuring, to an extent, emotional reactions. Indeed, previous research on bargaining games has shown that subjects' behaviour is different when using the strategy method than with the direct method (e.g. Brosig *et al.*, 2003; Guth *et al.*, 2001; Hoffman *et al.*, 1998; Schotter *et al.*, 1994). Brandts and Charness (2000), however, analysed a prisoner's dilemma and a game of chicken and found no difference. Concerning trust games, the only study that explored possible effects of the strategy method is the study by Casari and Cason (2009). They find that the strategy method lowered trustworthy behaviour. As this result would

have implications on the interpretations of our data, we explored if we could find similar strategy - direct method differences in our material.

We implemented the strategy method for the second mover in the AS03. In order to reduce complexity, we restricted the potential first movers' choices to which we asked second-movers to react, to three options, which were 0, 5, or 10 points. In the other AS and in SOEP 2003 we used the direct method. Thus, we can compare the level of trustworthiness with and without the strategy method. We find no difference in the average level of trustworthiness between the strategy method and the direct method (Figure D.2 and regression 2 in Table D.1). To make our results comparable with Casari and Cason (2009) we also look at the actual back-transfer rather than simply whether the back-transfer can be classified as trustworthy or not, and now we do find a small, significant effect; that is, second-movers' transfers are 0.3 points higher by strategy method compared to the direct method (regression 4 in Table D.1) - which is in fact the opposite of the findings by Casari and Cason (2009). This discrepancy raises questions as to what could be its causes. Since they used students as their subject pool, whereas we have nationally representative samples, this difference could be an explanation for the different results. However, our high back-transfer is not driven by a higher back-transfer or more trustworthy behaviour by non-students compared to students. Furthermore, in their trust game the truster had only a binary choice. Thus, trustees only had to decide for the case in which trusters fully trusted. We can test if the level of trust placed by first-movers influences the effect of the strategy method. For that reason, we compare second-movers' back-transfer for all three first-mover transfers separately. We find that the back-transfers for a first-mover transfer of 10 points is 0.78 points higher in the strategy method compared to the direct method (t-test = 2.67 $P < 0.01$). However, the average back-transfers for first-mover transfers of 0 and 5 points do not differ between the strategy and the direct method. Thus, the overall effect of the strategy method which we find is driven by a higher back-transfer when the first-mover fully trusts. The remaining difference between our test and the one by Casari and Cason (2009) is that their subjects only decided for one situation, that is, when the first-mover fully trusted. In our design, however, trustees had to decide in three different situations. Whether this drives the differences in our results remains an open question.

We conclude that the strategy method has no effects on the level of trustworthiness but increases second-movers' back-transfers in those cases, when first-movers placed full trust in them. This raises doubts for the use of the strategy method in trust game since decisions might be biased.

Table D.1: Sensitivity of experimental measure of trustworthiness

| Dependent variable: | dummy of being trustworthy | | Second-mover's back-transfer | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Dummy for design with 3 options | −0.009 | | −0.441 | |
| (Base: 11 options) | (0.040) | | (0.270) | |
| Dummy for design with 6 options | 0.027 | | −0.039 | |
| | (0.025) | | (0.156) | |
| Dummy for strategy method | | 0.010 | | 0.313$^{\star}$ |
| (Base: direct method) | | (0.024) | | (0.153) |
| Constant | 0.551$^{\star\star}$ | 0.530$^{\star\star}$ | 6.734$^{\star\star}$ | 6.320$^{\star\star}$ |
| | (0.023) | (0.024) | (0.156) | (0.166) |
| Controlled for first mover transfer | Yes | Yes | Yes | Yes |
| N | 1,625 | 1,333 | 2,181 | 1,908 |
| Adjusted $R^2$ | 0.127 | 0.160 | 0.215 | 0.220 |
| Cluster on individual level | Yes | Yes | Yes | Yes |

*Notes.* OLS regression, coefficients and robust standard errors (in parenthesis) reported. $^{\circ}$, $^{\star}$, $^{\star\star}$ denote statistical significance on the 10%, 5% and 1% level, respectively.
*Sources.* AS02, AS03, AS04, and SOEP 2003

# 2.E Participants' Instructions

## 2.E.1 Instructions for the Accompanying Study in 2002

**Informationen für Teilnehmer 1:**

In den letzten Jahren hat sich neben der Umfrageforschung ein neuer wichtiger Forschungszweig - die Verhaltensforschung - herausgebildet. Dabei wird das Verhalten von Menschen in Situationen untersucht, in denen man Entscheidungen über die Verwendung von Geldbeträgen treffen muss. Daraus kann man schließen, wie die Wirtschaft funktioniert.

Wir bitten Sie nun, eine derartige Entscheidung zu treffen. Das Geld für Ihre Entscheidung wird von der Wissenschaftsgemeinschaft Gottfried Wilhelm Leibniz bereitgestellt. Bei Ihrer Entscheidung müssen Sie beachten, dass Sie mit einem anderen - anonymen - Teilnehmer von „Leben in Deutschland" gepaart sind, d.h. Ihre Entscheidung und die Entscheidung des anderen Teilnehmers bestimmen zusammen, wie viel Sie und der andere letztlich verdienen. Das von Ihnen verdiente Geld zahlt Ihnen INFRATEST nach der Befragung aller Teilnehmer aus.

Bitte lesen Sie nun die Informationen zur Entscheidungssituation genau durch und treffen Sie dann Ihre Entscheidung.

Die Entscheidungssituation besteht aus zwei Teilnehmern: Teilnehmer 1 und Teilnehmer 2, die beide eine Entscheidung treffen, ohne sich zu kennen.

**Sie sind Teilnehmer 1.**

Jeder Teilnehmer erhält 10 Punkte Ausgangskapital.

Die Entscheidung jedes Teilnehmers besteht darin, das Ausgangskapital ganz oder teilweise für sich zu behalten bzw. ganz oder teilweise an den anderen Teilnehmer weiterzugeben.

Für jeden Punkt, den Sie für sich behalten, bekommen Sie genau 1 €, der andere nichts.
Für jeden Punkt, den Sie an den anderen weitergeben, bekommt er 2 €, Sie nichts.

Umgekehrt gilt dasselbe:
Für jeden Punkt, den der andere Teilnehmer behält, bekommt er 1 €, sie nichts.
Für jeden Punkt, den der andere an Sie weitergibt, bekommen Sie 2 €, der andere nichts.

Teilnehmer 1 beginnt:
d. h. Sie entscheiden wie viele Punkte Sie behalten bzw. an Teilnehmer 2 weitergeben.
Diese Entscheidung wird an Teilnehmer 2 weitergeleitet.

Dann muss dieser entscheiden, wie viele Punkte er für sich behält oder an Sie weitergibt.
Aus beiden Entscheidungen zusammen (der von Ihnen **und** der von Teilnehmer 2) ergibt sich das Einkommen, das Sie und Teilnehmer 2 erhalten.

Hier drei Beispiele:

- Jeder Teilnehmer behält seine 10 Punkte und gibt 0 Punkte an den anderen weiter. In diesem Fall erzielt jeder Teilnehmer ein Einkommen von 10 €.
- Jeder Teilnehmer behält nichts für sich selbst und gibt seine gesamten 10 Punkte an den anderen weiter. In diesem Fall erzielt jeder Teilnehmer ein Einkommen von 20 €.
- Teilnehmer 1 behält keine Punkte für sich und gibt die gesamten 10 Punkte an Teilnehmer 2 weiter. Teilnehmer 2 behält 8 Punkte für sich und gibt 2 Punkte an Teilnehmer 1 weiter.
  In diesem Fall erzielen die beiden Teilnehmer folgende Einkommen:

| | | | | | | |
|---|---|---|---|---|---|---|
| Einkommen von Teilnehmer 1 = | behaltene Punkte | + 2 x | erhaltene Punkte | | | |
| | | 0 P. | + 2 x | 2 P. | = | 4 € |
| Einkommen von Teilnehmer 2 = | behaltene Punkte | + 2 x | erhaltene Punkte | | | |
| | | 8 P. | + 2 x | 10 P. | = | 28 € |

**Um Ihre Punkte einzutragen, drehen Sie das Blatt bitte um.**

1

**Entscheidungsblatt für Teilnehmer 1**

**Sie sind Teilnehmer 1.**

Wie teilen Sie die 10 Punkte Ihres Ausgangskapitals auf?
Wie viele Punkte (von 0 bis 10) behalten Sie und wie viele geben Sie an Teilnehmer 2 weiter?

Ich behalte _____ Punkte und

gebe weiter _____ Punkte,

zusammen also: 10 Punkte.

Wie geht's weiter?

Infratest übermittelt nun die Punktezahl, die von Ihnen weitergegeben wurde, an Teilnehmer 2.

Teilnehmer 2 entscheidet dann, wie viele Punkte er an Sie gibt.

Hierzu noch eine Frage:
Was glauben Sie, wie viele Punkte wird Teilnehmer 2 an Sie transferieren?

_____ Punkte

Über das Ergebnis und das Einkommen, das an Sie ausbezahlt wird, werden Sie anschließend von INFRATEST informiert.

Bitte stecken Sie das ausgefüllte Blatt in den Briefumschlag und überreichen Sie den Umschlag dem Interviewer!

Einstweilen vielen Dank fürs Mitmachen!

2

## Informationen für Teilnehmer 2:

In den letzten Jahren hat sich neben der Umfrageforschung ein neuer wichtiger Forschungszweig - die Verhaltensforschung – herausgebildet. Dabei wird das Verhalten von Menschen in Situationen untersucht, in denen man Entscheidungen über die Verwendung von Geldbeträgen treffen muss. Daraus kann man schließen, wie die Wirtschaft funktioniert.

Wir bitten Sie nun, eine derartige Entscheidung zu treffen. Das Geld für Ihre Entscheidung wird von der Wissenschaftsgemeinschaft Gottfried Wilhelm Leibniz bereitgestellt. Bei Ihrer Entscheidung müssen Sie beachten, dass Sie mit einem anderen - anonymen - Teilnehmer von „Leben in Deutschland" gepaart sind, d.h. Ihre Entscheidung und die Entscheidung des anderen Teilnehmers bestimmen zusammen, wie viel Sie und der andere letztlich verdienen. Das von Ihnen verdiente Geld zahlt Ihnen INFRATEST nach der Befragung aller Teilnehmer aus.

Bitte lesen Sie nun die Informationen zur Entscheidungssituation genau durch und treffen Sie dann Ihre Entscheidung.

Die Entscheidungssituation besteht aus zwei Teilnehmern: Teilnehmer 1 und Teilnehmer 2, die beide eine Entscheidung treffen, ohne sich zu kennen.

**Sie sind Teilnehmer 2.**

Jeder Teilnehmer erhält 10 Punkte Ausgangskapital.

Die Entscheidung jedes Teilnehmers besteht darin, das Ausgangskapital ganz oder teilweise für sich zu behalten bzw. ganz oder teilweise an den anderen Teilnehmer weiterzugeben.

Für jeden Punkt, den Sie für sich behalten, bekommen Sie genau 1 €, der andere nichts.
Für jeden Punkt, den Sie an den anderen weitergeben, bekommt er 2 €, Sie nichts.

Umgekehrt gilt dasselbe:
Für jeden Punkt, den der andere Teilnehmer behält, bekommt er 1 €, Sie nichts.
Für jeden Punkt, den der andere an Sie weitergibt, bekommen Sie 2 €, der andere nichts.

Teilnehmer 1 hat begonnen:
d. h. er hat schon entschieden wie viele Punkte er behält bzw. an Sie weitergibt.
Das Ergebnis finden Sie auf der Rückseite.

Nun müssen Sie entscheiden, wie viele Punkte Sie für sich behalten oder an Teilnehmer 1 weitergeben. Aus beiden Entscheidungen zusammen (der von Teilnehmer 1 **und** der von Ihnen) ergibt sich das Einkommen, das Teilnehmer 1 und Sie erhalten.

Hier drei Beispiele:

- Jeder Teilnehmer behält seine 10 Punkte und gibt 0 Punkte an den anderen weiter. In diesem Fall erzielt jeder Teilnehmer ein Einkommen von 10 €.
- Jeder Teilnehmer behält nichts für sich selbst und gibt seine gesamten 10 Punkte an den anderen weiter. In diesem Fall erzielt jeder Teilnehmer ein Einkommen von 20 €.
- Teilnehmer 1 behält keine Punkte für sich und gibt die gesamten 10 Punkte an Teilnehmer 2 weiter. Teilnehmer 2 behält 8 Punkte für sich und gibt 2 Punkte an Teilnehmer 1 weiter.
  In diesem Fall erzielen die beiden Teilnehmer folgende Einkommen:

| | | | | | | |
|---|---|---|---|---|---|---|
| Einkommen von Teilnehmer 1 = | behaltene Punkte | + 2 x | erhaltene Punkte | | | |
| | 0 P. | | + 2 x | 2 P. | = | 4 € |
| Einkommen von Teilnehmer 2 = | behaltene Punkte | + 2 x | erhaltene Punkte | | | |
| | 8 P. | | + 2 x | 10 P. | = | 28 € |

**Um Ihre Punkte einzutragen, drehen Sie das Blatt bitte um.**

3

**Entscheidungsblatt für Teilnehmer 2**

**Sie sind Teilnehmer 2, das heißt Teilnehmer 1 hat seine Entscheidung bereits getroffen.**

Die Punkteanzahl, die Teilnehmer 1 an Sie weitergegeben hat, beträgt:

_____ Punkte.

Wie teilen Sie nun die 10 Punkte Ihres Ausgangskapitals auf?
Wie viele Punkte (von 0 bis 10) behalten Sie und wie viele geben Sie an Teilnehmer 1 weiter?

Ich behalte _____ Punkte und

gebe weiter _____ Punkte,

zusammen also: 10 Punkte.

Wie geht's weiter?

Infratest stellt nun das Ergebnis fest und wird das erzielte Einkommen an beide Teilnehmer auszahlen.

Bitte stecken Sie das ausgefüllte Blatt in den Briefumschlag und überreichen Sie den Umschlag dem Interviewer!

Einstweilen vielen Dank fürs Mitmachen!

4

## 2.E.2 Instructions for the Accompanying Study in 2003

Infratest Sozialforschung Deutsches Institut für Wirtschaftsforschung

# Leben in Deutschland

### Verhaltensexperiment „Geben und Nehmen"

Zwei Personen,
die sich nicht kennen,
treffen jeder eine Entscheidung
über die Verwendung von Geld
und erzielen zusammen ein Ergebnis.

**Spielregeln: So wird's gemacht!**

Es gibt zwei Teilnehmer.
Jeder Teilnehmer erhält einen Anfangsbetrag von 10 Punkten.

Beide Teilnehmer können ihre 10 Punkte

- zwischen sich und der anderen Person aufteilen oder
- ganz für sich behalten oder
- ganz an die andere Person weitergeben.

Für jeden Punkt, den Sie behalten, bekommen Sie selbst 1 €. 

Für jeden Punkt, den Sie weitergeben, bekommt die andere Person 2 €. 

Umgekehrt gilt:
Für jeden Punkt, den die andere Person an Sie weitergibt, bekommen Sie 2 €

**Für Teilnehmer1 bedeutet:**
**Behalten: 0 Punkte = 0€ / 5 Punkte = 5€ / 10 Punkte = 10€**
**Weitergeben: 0 Punkte = 0€ / 5 Punkte = 10€ / 10 Punkte = 20€**

**Für Teilnehmer2 bedeutet:**
**Behalten: 0 Punkte = 0€ / 1 Punkt = 1€ / 2 Punkte = 2€ / 3 Punkte = 3€ *usw. bis* / 10 Punkte = 10€**
**Weitergeben: 0 Punkte = 0€ / 1 Punkt = 2€ / 2 Punkte = 4€ / 3 Punkte = 6€ *usw. bis* / 10 Punkte = 20€**

**Sie sind Teilnehmer 1, entscheiden also als Erster.**

Das heißt, Sie entscheiden, ob Sie 10 oder 5 oder 0 Punkte an die andere Person (Teilnehmer 2) weitergeben und wie viele Sie behalten.

Danach wird Teilnehmer 2 seine Entscheidung über die 10 Punkte treffen. Er wird nie erfahren wer Sie sind, aber wir werden ihn über Ihre Entscheidung informieren. Teilnehmer 2 kann eine beliebige Punktezahl zwischen 0 und 10 an Sie weitergeben.

Aus beiden Entscheidungen zusammen errechnet sich der Geldbetrag, den Sie erzielen. Diesen Geldbetrag werden wir Ihnen per Verrechnungsscheck zusenden. Ihre Teilnahme ist freiwillig. Der Rechtsweg ist ausgeschlossen.

**Um Ihre Punkte einzutragen, drehen Sie das Blatt bitte um.** ➡

- 1 -

## Entscheidungsblatt für Teilnehmer 1

Wie teilen Sie die **10 Punkte** auf?

Das heißt, wie viele Punkte **(10 oder 5 oder 0)** geben Sie an Teilnehmer 2 weiter und wie viele behalten Sie?

| Ich gebe 10 Punkte weiter und behalte 0 Punkte | | Ich gebe 5 Punkte weiter und behalte 5 Punkte | | Ich gebe 0 Punkte weiter und behalte 10 Punkte |
|---|---|---|---|---|
| ☐ | oder | ☐ | oder | ☐ |

**Und so geht's weiter:**

Jetzt entscheidet Teilnehmer 2 wie viele Punkte er an Sie gibt.

Hierzu noch eine Frage:
Was denken Sie, wie werden sich die Teilnehmer 2 in der Regel verhalten?

Teilnehmer 2, die 10 Punkte erhalten, geben in der Regel ☐ Punkte

Teilnehmer 2, die 5 Punkte erhalten, geben in der Regel ☐ Punkte

Teilnehmer 2, die 0 Punkte erhalten, geben in der Regel ☐ Punkte

---

Haben Sie die drei oberen Kästchen ausgefüllt?

**Dann sagen Sie uns bitte noch:**

Wie gerne nehmen Sie an einer solchen Entscheidungsaufgabe teil?

sehr gerne ☐ weniger gerne ☐ ungern ☐

Wie verständlich haben wir die Entscheidungsaufgabe erklärt?

sehr gut ☐ weniger gut ☐ schlecht ☐

Nun stecken Sie bitte das Blatt in den Briefumschlag und überreichen Sie den **verschlossenen** Umschlag dem Interviewer!

**Vielen Dank fürs Mitmachen!**

- 2 -

Infratest Sozialforschung Deutsches Institut für Wirtschaftsforschung

# Leben in Deutschland

## Verhaltensexperiment „Geben und Nehmen“

Zwei Personen,
die sich nicht kennen,
treffen jeder eine Entscheidung
über die Verwendung von Geld
und erzielen zusammen ein Ergebnis.

### Spielregeln: So wird's gemacht!

Es gibt zwei Teilnehmer.
Jeder Teilnehmer erhält einen Anfangsbetrag von 10 Punkten.

Beide Teilnehmer können ihre 10 Punkte
- zwischen sich und der anderen Person aufteilen oder
- ganz für sich behalten oder
- ganz an die andere Person weitergeben.

Für jeden Punkt, den Sie behalten, bekommen Sie selbst 1 €. 

Für jeden Punkt, den Sie weitergeben, bekommt die andere Person 2 €. 

Umgekehrt gilt:
Für jeden Punkt, den die andere Person an Sie weitergibt, bekommen Sie 2 €

**Für Teilnehmer1 bedeutet:**
**Behalten: 0 Punkte = 0€ / 5 Punkte = 5€ / 10 Punkte = 10€**
**Weitergeben: 0 Punkte = 0€ / 5 Punkte = 10€ / 10 Punkte = 20€**

**Für Teilnehmer2 bedeutet:**
**Behalten: 0 Punkte = 0€ / 1 Punkt = 1€ / 2 Punkte = 2€ / 3 Punkte = 3€ *usw. bis* / 10 Punkte = 10€**
**Weitergeben: 0 Punkte = 0€ / 1 Punkt = 2€ / 2 Punkte = 4€ / 3 Punkte = 6€ *usw. bis* / 10 Punkte = 20€**

Teilnehmer 1 hat begonnen, er hat schon entschieden, ob er entweder 0 oder 5 oder 10 Punkte an Sie weitergibt.

**Sie sind Teilnehmer 2, entscheiden also als Zweiter.**

Jetzt können Sie Ihre Entscheidung über die 10 Punkte treffen. Sie können jede Punktezahl von 0 bis 10 an die andere Person (Teilnehmer 1) weitergeben. Da Sie noch nicht wissen wie sich Teilnehmer 1 entschieden hat, geben Sie für alle drei möglichen Entscheidungen von Teilnehmer 1 an, wie viel Punkte Sie weitergeben.

Aus der Entscheidung von Teilnehmer 1 und Ihrer entsprechenden Entscheidung errechnet sich der Geldbetrag, den Sie erzielen. Diesen Geldbetrag werden wir Ihnen per Verrechnungsscheck zusenden.
Ihre Teilnahme ist freiwillig. Der Rechtsweg ist ausgeschlossen.

**Um Ihre Punkte einzutragen, drehen Sie das Blatt bitte um.** ➡

- 1 -

## Entscheidungsblatt für Teilnehmer 2

**Sie sind Teilnehmer 2**, das heißt Teilnehmer 1 hat seine Entscheidung bereits getroffen.

Jetzt sind Sie an der Reihe und entscheiden wie Sie die **10 Punkte (von 0 bis 10)** aufteilen wenn Teilnehmer1 an Sie, 0 oder 5 oder 10 Punkte weitergegeben hat.

Wenn Teilnehmer 1 an mich **0 Punkte** weitergegeben hat,

dann gebe ich ☐ Punkte
und behalte ☐ Punkte
zusammen also 10 Punkte.

Wenn Teilnehmer 1 an mich **5 Punkte** weitergegeben hat,

dann gebe ich ☐ Punkte
und behalte ☐ Punkte
zusammen also 10 Punkte.

Wenn Teilnehmer 1 an mich **10 Punkte** weitergegeben hat,

dann gebe ich ☐ Punkte
und behalte ☐ Punkte
zusammen also 10 Punkte.

**Und so geht's weiter:**

Haben Sie die sechs oberen Kästchen ausgefüllt?

**Dann sagen Sie uns bitte noch:**

Wie gerne nehmen Sie an einer solchen Entscheidungsaufgabe teil?

sehr gerne ☐ weniger gerne ☐ ungern ☐

Wie verständlich haben wir die Entscheidungsaufgabe erklärt?

sehr gut ☐ weniger gut ☐ schlecht ☐

Nun stecken Sie bitte das Blatt in den Briefumschlag und überreichen Sie den **verschlossenen** Umschlag dem Interviewer!

**Vielen Dank fürs Mitmachen!**

- 2 -

## 2.E.3 Instructions for the Accompanying Study in 2004

Infratest Sozialforschung Deutsches Institut für Wirtschaftsforschung

# Leben in Deutschland

### Verhaltensexperiment „Geben und Nehmen"

Zwei Personen,
die sich nicht kennen,
treffen jeder eine Entscheidung
über die Verwendung von Geld
und erzielen zusammen ein Ergebnis.

### Spielregeln: So wird's gemacht!

Es gibt zwei Teilnehmer.
Jeder Teilnehmer erhält einen Anfangsbetrag von 10 Punkten.

Beide Teilnehmer können ihre 10 Punkte
- zwischen sich und der anderen Person aufteilen oder
- ganz für sich behalten oder
- ganz an die andere Person weitergeben.

Für jeden Punkt, den Sie behalten, bekommen Sie selbst 1 €. 

Für jeden Punkt, den Sie weitergeben, bekommt die andere Person 2 €. 

Umgekehrt gilt:
Für jeden Punkt, den die andere Person an Sie weitergibt, bekommen Sie 2 €

**Für beide Teilnehmer bedeutet:**
**Behalten: 0 Punkte = 0€ / 2 Punkte = 2€ / 4 Punkte = 4€ / 6 Punkte = 6€ / 8 Punkte = 8€ / 10 Punkte = 10€**
**Weitergeben: 0 Punkte = 0€ / 2 Punkte = 4€ / 4 Punkte = 8€ / 6 Punkte = 12€ / 8 Punkte = 16€ / 10 Punkte = 20€**

**Sie sind Teilnehmer 1, entscheiden also als Erster.**

Das heißt, Sie entscheiden, ob Sie 10, 8, 6, 4, 2, oder 0 Punkte an die andere Person (Teilnehmer 2) weitergeben und wie viele Sie behalten.

Danach wird Teilnehmer 2 seine Entscheidung über die 10 Punkte treffen. Er wird nie erfahren wer Sie sind, aber wir werden ihn über Ihre Entscheidung informieren.

Aus beiden Entscheidungen zusammen errechnet sich der Geldbetrag, den Sie erzielen.
Diesen Geldbetrag werden wir Ihnen per Verrechnungsscheck zusenden.
Ihre Teilnahme ist freiwillig. Der Rechtsweg ist ausgeschlossen.

**Um Ihre Punkte einzutragen, drehen Sie das Blatt bitte um.** ➡

- 1 -

## Entscheidungsblatt für Teilnehmer 1

Wie teilen Sie die **10 Punkte** auf?

Das heißt, wie viele Punkte **(10 oder 8 oder 6 oder 4 oder 2 oder 0)** geben Sie an Teilnehmer 2 weiter und wie viele behalten Sie?

*Bitte kreuzen Sie das entsprechende Kästchen an!*

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| Ich gebe | **10** | **8** | **6** | **4** | **2** | **0** | Punkte weiter, |
| also behalte ich | **0** | **2** | **4** | **6** | **8** | **10** | Punkte |
| | ☐ | ☐ | ☐ | ☐ | ☐ | ☐ | |

**Und so geht's weiter:**

Infratest übermittelt nun die Punktezahl, die von Ihnen weitergegeben wurde, an Teilnehmer 2.

Teilnehmer 2 entscheidet daraufhin, wie viele Punkte er an Sie gibt.

„Was wäre wenn …" Die bekannte Frage aus dem Alltag bewegt auch die Wissenschaft. Auch im Wirtschaftsleben ist es häufig notwendig, vorherzusagen, wie sich andere Menschen verhalten. Daher bitten wir Sie, hier noch eine Zusatzfrage dazu zu beantworten:

Was denken Sie, wie würde sich Teilnehmer 2 in den zwei folgenden Beispielen verhalten?

Wenn ich ihm 10 Punkte gebe, erhalte ich von ihm ☐ Punkte

Wenn ich ihm 0 Punkte gebe, erhalte ich von ihm ☐ Punkte

---

Haben Sie auch die beiden unteren Kästchen ausgefüllt?

Dann stecken Sie bitte das Blatt in den Briefumschlag und überreichen Sie den **verschlossenen** Umschlag dem Interviewer!

**Vielen Dank fürs Mitmachen!**

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# Leben in Deutschland

## Verhaltensexperiment „Geben und Nehmen“

Zwei Personen,
die sich nicht kennen,
treffen jeder eine Entscheidung
über die Verwendung von Geld
und erzielen zusammen ein Ergebnis.

### Spielregeln: So wird's gemacht!

Es gibt zwei Teilnehmer.
Jeder Teilnehmer erhält einen Anfangsbetrag von 10 Punkten.

Beide Teilnehmer können ihre 10 Punkte
- zwischen sich und der anderen Person aufteilen oder
- ganz für sich behalten oder
- ganz an die andere Person weitergeben.

Für jeden Punkt, den Sie behalten, bekommen Sie selbst 1 €. 

Für jeden Punkt, den Sie weitergeben, bekommt die andere Person 2 €. 

Umgekehrt gilt:
Für jeden Punkt, den die andere Person an Sie weitergibt, bekommen Sie 2 €

**Dies bedeutet für beide Teilnehmer:**
**Behalten: 0 Punkte = 0€ / 2 Punkte = 2€ / 4 Punkte = 4€ / 6 Punkte = 6€ / 8 Punkte = 8€ / 10 Punkte = 10€**
**Weitergeben: 0 Punkte = 0€ / 2 Punkte = 4€ / 4 Punkte = 8€ / 6 Punkte = 12€ / 8 Punkte = 16€ / 10 Punkte = 20€**

**Sie sind Teilnehmer 2, entscheiden also als Zweiter.**

Teilnehmer 1 hat begonnen, er hat schon entschieden, wie viele Punkte er behält bzw. an Sie weitergibt. Das Ergebnis finden Sie auf der Rückseite.

Jetzt sind Sie an der Reihe, das heißt, Sie entscheiden, wie viele von Ihren 10 Punkten Sie an die andere Person (Teilnehmer 1) weitergeben und wie viele Sie behalten. Sie haben die Wahl zwischen 10, 8, 6, 4, 2 oder 0 Punkten.

Aus beiden Entscheidungen zusammen errechnet sich der Geldbetrag, den Sie erzielen. Diesen Geldbetrag werden wir Ihnen per Verrechnungsscheck zusenden.
Ihre Teilnahme ist freiwillig. Der Rechtsweg ist ausgeschlossen.

**Um Ihre Punkte einzutragen, drehen Sie das Blatt bitte um.** ➡

- 1 -

## Entscheidungsblatt für Teilnehmer 2

**Sie sind Teilnehmer 2**, das heißt Teilnehmer 1 hat seine Entscheidung bereits

getroffen und hat Ihnen ☐ Punkte gegeben.

Jetzt sind Sie an der Reihe.

Wie teilen Sie **Ihre 10 Punkte** auf?

Das heißt, wie viele Punkte **(10 oder 8 oder 6 oder 4 oder 2 oder 0)** geben Sie an Teilnehmer 1 weiter und wie viele behalten Sie?

*Bitte kreuzen Sie das entsprechende Kästchen an!*

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| Ich gebe | **10** | **8** | **6** | **4** | **2** | **0** | Punkte weiter, |
| also behalte ich | **0** | **2** | **4** | **6** | **8** | **10** | Punkte |
| | ☐ | ☐ | ☐ | ☐ | ☐ | ☐ | |

Haben Sie ein Kästchen angekreuzt?

Dann stecken Sie bitte das Blatt in den Briefumschlag und überreichen Sie den **verschlossenen** Umschlag dem Interviewer!

**Vielen Dank fürs Mitmachen!**

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## 2.E.4 Instructions for the SOEP 2003

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# Leben in Deutschland

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**Spielregeln: So wird's gemacht!**

Es gibt zwei Teilnehmer.
Jeder Teilnehmer erhält einen Anfangsbetrag von 10 Punkten.

Beide Teilnehmer können ihre 10 Punkte
- zwischen sich und der anderen Person aufteilen oder
- ganz für sich behalten oder
- ganz an die andere Person weitergeben.

Für jeden Punkt, den Sie behalten, bekommen Sie selbst 1 €. 

Für jeden Punkt, den Sie weitergeben, bekommt die andere Person 2 €. 

Umgekehrt gilt:
Für jeden Punkt, den die andere Person an Sie weitergibt, bekommen Sie 2 €

**Also bedeutet für beide Teilnehmer:**
**Behalten: 0 Punkte = 0€ / 1 Punkt = 1€ / 2 Punkte = 2€ / 3 Punkte = 3€ *usw. bis* / 10 Punkte = 10€**
**Weitergeben: 0 Punkte = 0€ / 1 Punkt = 2€ / 2 Punkte = 4€ / 3 Punkte = 6€ *usw. bis* / 10 Punkte = 20€**

**Sie sind Teilnehmer 1, entscheiden also als Erster.**

Das heißt, Sie entscheiden, wie viele von Ihren 10 Punkten Sie an die andere Person (Teilnehmer2) weitergeben und wie viele Sie behalten.

Teilnehmer 2 wird nie erfahren wer Sie sind, aber wir werden Ihre Entscheidung an diese Person übermitteln.

Teilnehmer 2 weiß also, wie viele Punkte Sie ihm gegeben haben, wenn er nun seine Entscheidung über die Aufteilung der 10 Punkte trifft.

Aus beiden Entscheidungen zusammen errechnet sich der Geldbetrag, den Sie erzielen. Diesen Geldbetrag werden wir Ihnen per Verrechnungsscheck zusenden. Ihre Teilnahme ist freiwillig. Der Rechtsweg ist ausgeschlossen.

**Um Ihre Punkte einzutragen, drehen Sie das Blatt bitte um.** ➡

- 1 -

## Entscheidungsblatt für Teilnehmer 1

Wie teilen Sie die **10 Punkte** auf?

Das heißt, wie viele Punkte **(von 0 bis 10)** geben Sie an Teilnehmer 2 weiter und wie viele behalten Sie?

*Ich gebe* ☐ *Punkte weiter*

*und behalte* ☐ *Punkte*

zusammen also 10 Punkte.

**Und so geht's weiter:**

Infratest übermittelt nun die Punktezahl, die von Ihnen weitergegeben wurde, an Teilnehmer 2.

Teilnehmer 2 entscheidet daraufhin, wie viele Punkte er an Sie gibt.

Hierzu noch eine Frage:
Was denken Sie, wie viele Punkte wird Teilnehmer 2 an Sie geben?

*Teilnehmer 2 wird* ☐ *Punkte an mich geben.*

---

Haben Sie die drei oberen Kästchen ausgefüllt?

**Dann sagen Sie uns bitte noch:**

Wie gerne nehmen Sie an einer solchen Entscheidungsaufgabe teil?

sehr gerne ☐ weniger gerne ☐ ungern ☐

Wie verständlich haben wir die Entscheidungsaufgabe erklärt?

sehr gut ☐ weniger gut ☐ schlecht ☐

Nun stecken Sie bitte das Blatt in den Briefumschlag und überreichen Sie den **verschlossenen** Umschlag dem Interviewer!

**Vielen Dank fürs Mitmachen!**

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### Spielregeln: So wird's gemacht!

Es gibt zwei Teilnehmer.
Jeder Teilnehmer erhält einen Anfangsbetrag von 10 Punkten.

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- zwischen sich und der anderen Person aufteilen oder
- ganz für sich behalten oder
- ganz an die andere Person weitergeben.

Für jeden Punkt, den Sie behalten, bekommen Sie selbst 1 €. 

Für jeden Punkt, den Sie weitergeben, bekommt die andere Person 2 €. 

Umgekehrt gilt:
Für jeden Punkt, den die andere Person an Sie weitergibt, bekommen Sie 2 €

**Also bedeutet für beide Teilnehmer:**
**Behalten: 0 Punkte = 0€ / 1 Punkt = 1€ / 2 Punkte = 2€ / 3 Punkte = 3€ *usw. bis* / 10 Punkte = 10€**
**Weitergeben: 0 Punkte = 0€ / 1 Punkt = 2€ / 2 Punkte = 4€ / 3 Punkte = 6€ *usw. bis* / 10 Punkte = 20€**

**Sie sind Teilnehmer 2, entscheiden also als Zweiter.**

Teilnehmer 1 hat begonnen, er hat schon entschieden, wie viele Punkte er behält bzw. an Sie weitergibt. Das Ergebnis finden Sie auf der Rückseite.

Jetzt sind Sie an der Reihe, das heißt, Sie entscheiden, wie viele von Ihren 10 Punkten Sie an die andere Person (Teilnehmer1) weitergeben und wie viele Sie behalten.

Aus beiden Entscheidungen zusammen errechnet sich der Geldbetrag, den Sie erzielen. Diesen Geldbetrag werden wir Ihnen per Verrechnungsscheck zusenden. Ihre Teilnahme ist freiwillig. Der Rechtsweg ist ausgeschlossen.

**Um Ihre Punkte einzutragen, drehen Sie das Blatt bitte um.** ➡

- 1 -

## Entscheidungsblatt für Teilnehmer 2

**Sie sind Teilnehmer 2**, das heißt Teilnehmer 1 hat seine Entscheidung bereits

getroffen und hat Ihnen ☐ Punkte gegeben.

Wie teilen Sie die **10 Punkte** auf?

Das heißt, wie viele Punkte **(von 0 bis 10)** geben Sie an Teilnehmer 1 weiter und wie viele behalten Sie?

*Ich gebe* ☐ *Punkte weiter*

*und behalte* ☐ *Punkte*

zusammen also 10 Punkte.

**Und so geht's weiter:**

Haben Sie die beiden oberen Kästchen ausgefüllt?

**Dann sagen Sie uns bitte noch:**

Wie gerne nehmen Sie an einer solchen Entscheidungsaufgabe teil?

sehr gerne ☐ weniger gerne ☐ ungern ☐

Wie verständlich haben wir die Entscheidungsaufgabe erklärt?

sehr gut ☐ weniger gut ☐ schlecht ☐

Nun stecken Sie bitte das Blatt in den Briefumschlag und überreichen Sie den **verschlossenen** Umschlag dem Interviewer!

**Vielen Dank fürs Mitmachen!**

- 2 -

CHAPTER

# THREE

# DECOMPOSING TRUST: EXPLAINING NATIONAL AND ETHNIC TRUST DIFFERENCES

Joint with Ernst Fehr, Urs Fischbacher,
Jürgen Schupp and Gert G. Wagner

## 3.1 Introduction

The level of trust is not the same for every person nor is the aggregate level of trust similar in different countries. In their seminal papers, Knack and Keefer (1997) and La Porta *et al.* (1997) have demonstrated that country differences in the aggregate level of trust are related to important economic variables such as growth and inflation in those countries. As mentioned in section 1.4 there is little research on what explains differences in trust on national level. This is especially intriguing as national differences in trust may well be at the core of understanding differences in economic developments and between changes of economic systems. One of many examples for the importance of trust may well be the recent credit crunch and the mistrust between banks.

We largely follow James Coleman's concept of trust (Coleman, 1990) which is well suited for behavioural sciences such as economics. In his perspective, the following two points characterise the action of placing trust. On the one hand it implies that the truster freely transfers assets to another person, without controlling the actions of that other person or having the possibility to retaliate. Second, there must be a potential gain in order to have an incentive to trust. The incentive is such that if the other person is trustworthy, the truster is better off trusting than not having trusted: and worse off if the other person does not fulfil the trust placed in him/her. There is no monetary incentive for the second-mover to reward the first-mover's trusting behaviour with trustworthiness.

Note that in this concept, trust is considered as a form of behaviour. Thus, following this concept, using experimental methods to measure trust suggests itself (Berg *et al.*, 1995).

This paper uses novel empirical methods with the aim of shedding light on some interesting aspects of trust. For this purpose, we developed a new empirical approach that enables us to combine large non-interactive surveys with interactive experiments. Given the relevance of trust in economic and political relations, we explore if levels of trust and trustworthiness differ between countries; and how any such differences can be explained. We also illuminate differences in trust within a country, along demographic, mainly ethnic variables. Our choice to compare Germany and the United States (U.S.) reflects the interesting role which trust plays in the performance of the economy and the different institutions in these countries (Alesina *et al.*, 2001). Some indications as to expected differences in levels of trust between countries might be found in survey data. Measured by questionnaire in a representative sample of respondents, the World Values Survey allows conclusions about the generalised expectancies for interpersonal trust on national levels. The levels of trust found therein are higher in the United States than in Germany for 1990, slightly higher in Germany for 1999, and slightly higher in the United States in 2006. A representative study of elderly people in Germany and the United States in 2000/2001, however, shows a slightly higher level of expectancies for neighbourhood-related trust in the United States than in Germany (Pollack and von dem Knesebeck, 2004). Regarding differences within countries, a quick analysis of World Value Survey data for 2006 shows that the share of the Caucasian populace with positive expectancies for interpersonal trust is more than double that of the African American populace. Similar results for ethnic differences are found in the General Social Survey (Glaeser *et al.*, 2000). However, serious doubts have been raised about the validity of the survey measure of trust used in the World Values Survey and the General Social Survey and especially about the cross-cultural comparison (Glaeser *et al.*, 2000; Miller and Mitamura, 2003; Reeskens and Hooghe, 2008). Thus, a measure of trust other than the commonly used trust question in these surveys is desirable.

The second question we address is why certain people trust more than others or why people in a certain country behave more trustingly than people in other countries. What are the factors that influence whether a person trusts or mistrusts? Previous literature suggests that risk preferences (Schechter, 2007), social preferences (Bohnet and Zeckhauser, 2004; Bohnet *et al.*, 2008), expectations about others' trustworthiness (Ashraf *et al.*, 2006; Bohnet and Zeckhauser, 2004) and life experiences and life expectancy measured by demographic variables such as age (Bellemare and Kroeger, 2007; Sutter and Kocher, 2007), education (Bellemare and Kroeger, 2007) income (Alesina and La Ferrara,

2002; Bellemare and Kroeger, 2007) and religion (Guiso *et al.*, 2003) are determinants of trusting behaviour. For our analysis we take up these suggested factors and improve upon them by exploring models including several rather than single factors.

In this paper we combine two national representative surveys with a trust experiment. This approach allows us, on the one hand, to simultaneously analyse the influence of a large variety of personal characteristics on trusting behaviour. On the other hand, in contrast to many studies based on experiments conducted with student populations, we have the opportunity to analyse trusting behaviour of the general populace of two countries. This has the advantage that the heterogeneity of our subjects' behaviour and characteristics is much larger than with a student population.

Thanks to this new approach, we are able to conclusively demonstrate a significant difference in the level of trust between Germany and the United States – people in the United States trusting more than in Germany, on average. We further illustrate that the factors that were suggested to influence trust, i.e. risk preferences, social preferences, and expected trustworthiness, do indeed correlate strongly with levels of trust – individually as well as when controlling for the other factors. We were also able to show that these factors are found in differing levels in the two countries. U.S. Americans are less risk averse, less betrayal averse and more optimistic about others' trustworthiness. Now, if factors that explain trust were distributed differently in our two countries, this would serve to explain the differing trust levels found. Indeed, running multivariate regression models across our data, we show that the national differences in trust are almost entirely explained by the national differences in social and risk preferences and expected trustworthiness. In contrast, neither the range of demographic variables that we tested, nor income differences, had a relevant part in explaining the trust gap (social and risk preferences and expected trustworthiness explain 81%, demographic variables 7% and income 7% of the gap).

As our methods allowed us to acquire representative data for countries, we were also able to explore some variations of trust levels within the United States. Particularly, we found that Caucasian Americans exhibit a 72% higher level of trust than African Americans, whilst Latinos and Latinas trust not quite as much, but still 48% more than African Americans. In contrast to the difference between countries, we were only able to explain 40% of this ethnic trust gap within the United States. Furthermore, income is of much higher relevance for explaining this trust gap than it is for the gap between Germany and the United States. On the other hand, the effect of income together with our explanatory variables, comprising risk and social preferences as well as expected trustworthiness, do still account for 96% of the explainable ethnic trust gap.

The remainder of the paper is organised as follows: in the next section we describe

the design of the experiment and the survey and show that our procedures are suitable for procuring representative data. In sections 3.3 - 3.8 we present our results. Section 3.9 summarises and concludes the paper.

## 3.2 Design

We measure trust through a trust game, played anonymously with real monetary stakes (Berg *et al.*, 1995; Camerer and Weigelt, 1988; Coleman, 1990). In the trust game two players receive an endowment of ten US$ or Euros from the experimenters and interact in the following way. The first-mover may voluntarily place trust in the second-mover (a person unknown to him/her), by transferring zero to ten US$ of his or her endowment. This money will then be at the disposal of the second-mover, without any commitment from the second-mover to the first-mover. Thus, according to our concept of trust, the first-mover becomes a truster, whereas the second-mover is trusted, so we will name him/her 'trustee'. Every transfer is doubled by the experimenters and thus increases the total amount earned for the trustee. At the next stage of the game, the trustee is informed about the transfer he/she has received from the truster and then decides on how much to transfer back. The transfer is again between zero and ten US$ and the experimenters again double the amount transferred. After the second-mover's decision, the game is over and both players are paid the amount earned. There are no other rounds of interaction between the two players. The design of the experiment is kept simple in order to make sure that everybody in the population has a chance to understand the rules of the game and calculate the payoffs in their head. The symmetry of the game is a consequence of these limitations but also a design feature. Contrary to the trust game usually used (Berg *et al.*, 1995), the strategy space of second-movers is independent of the decision of first-movers, for this reason a positive relation between received and returned money cannot be due to a larger available strategy. The experimental instructions can be found in appendix 3.A. If the truster places full trust in the trustee – that is, if he/she transfers the whole endowment of ten US$ – and the trustee behaves other-regardingly and fully honours the placement of trust by re-transferring the whole endowment of ten US$, then the overall income is maximised such that both players end up with twenty US$. On the other hand, by behaving selfishly and not transferring anything back, the trustee is able to increase his/her earnings to thirty US$ while leaving the truster with nothing. The first-mover's dilemma therefore is between a) transferring a positive amount and perhaps earning more than ten US$, while bearing the risk of losing some or all of his/her original endowment of ten US$, and b) taking the guaranteed award of ten US$ by not transferring anything. Thus,

the outcome depends on the action of the trustee. The interpretation of a positive transfer by the truster as placement of trust in the trustee is thus compatible with the concept of trust by James Coleman (Coleman, 1990) given above. To collect a large amount of high quality, representative data including an interactive experiment is challenging. Thus we cooperated with two well-known, experienced institutions, one in each country. In Germany, we used the German Socio-Economic Panel (SOEP), a longitudinal study, as the framework for our experiment. The SOEP is located at the German Institute of Economic Research (DIW Berlin) and funded by the Leibniz society, a research organisation for applied sciences. A subsample of 1,202 SOEP respondents was selected to participate in the experiment[1]. After the normal SOEP questionnaire, the interviewer handed over the written instructions to participants. The interviewers were briefed to give detailed information to the participants if needed before they made their decisions. Then the participants were asked to privately make their decision and put the decision sheet into an envelope and close it. Thus the interviewer does not know and will never get to know how the participant decided. The feedback and a check for the amount earned were mailed to the subjects a few weeks later together with a thank-you letter. One might worry that respondents did not believe that they would eventually receive the money. Thus, respondents needed to believe that the survey organisation would keep its promises. Since respondents are in this panel for several years, they experienced several interactions with the survey organisation. Most importantly for our purpose is that the survey respondents are promised a lottery ticket for a national charity lottery as an incentive for participation in the yearly survey. This ticket is mailed to them only after the interview took place. Thus the respondents repeatedly experienced that the survey organisation would keep its promise and that is the reason why we think respondents did not doubt that the money would be paid out. The SOEP is a representative sample of the entire German population (Wagner *et al.*, 2007). Our randomly drawn subsample is thus representative for Germany as well. Nevertheless, we compare socio-demographic characteristics of the participants in the experiment with the total SOEP sample of 21,105 individuals. The weighted[2] distribution of the participants closely follows that of the entire SOEP population (Table 3.1) that assures us of the representativity of our subsample.

In the United States we used the facilities of Knowledge Networks (KN) who run

[1]Only persons above the age of 18 and only one person per household is selected and 90% of the selected persons could be fielded between February and May 2005 and the rest before October 2005.

[2]The weights are based on the distributions of geographical position, the size of the household, gender, age, and nationality compared to the distributions in the German Census. These weights are then adjusted every year with the determinants of the dropouts in that year.

Table 3.1: Demographic description of the data (in percent)

| | | United States | | | Germany | | |
|---|---|---|---|---|---|---|---|
| | | CPS | first mover | second mover | SOEP | first mover | second mover |
| Gender | male | 48.1 | 48.2 | 48.0 | 46.1 | 50.2 | 46.6 |
| | female | 51.9 | 51.8 | 52.0 | 53.9 | 49.8 | 53.4 |
| Age | 18 - 30 | 23.5 | 23.5 | 23.2 | 18.5 | 14.2 | 16.6 |
| | 31 - 50 | 39.5 | 39.4 | 40.6 | 37.5 | 36.2 | 34.9 |
| | 51 - 103 | 37.0 | 37.1 | 36.2 | 44.0 | 49.6 | 48.5 |
| Household income | below 10,000 US$ | 6.8 | 11.3 | 10.2 | 8.9 | 6.7 | 9.3 |
| | 10,000 - 24,999 | 17.3 | 18.4 | 18.2 | 30.2 | 31.8 | 33.3 |
| | 25,000 - 49,999 | 28.3 | 31.0 | 33.5 | 35.3 | 38.0 | 35.9 |
| | 50,000 - 74,999 | 20.1 | 21.3 | 18.2 | 15.8 | 16.6 | 16.5 |
| | 75,000 and more | 27.5 | 18.1 | 19.8 | 9.8 | 6.9 | 5.0 |
| Household size | | 2.9 | 2.9 | 2.9 | 2.5 | 2.5 | 2.4 |
| Education | less than high school | 16.0 | 15.6 | 15.9 | 21.0 | 20.3 | 20.9 |
| | high school | 58.7 | 60.0 | 59.9 | 59.6 | 64.7 | 65.8 |
| | more than high school | 25.3 | 24.4 | 24.2 | 19.4 | 14.9 | 13.3 |
| Employment status | employed | 56.6 | 48.2 | 48.7 | 47.3 | 44.3 | 46.1 |
| | unemployed | 3.2 | 6.6 | 4.8 | 7.3 | 6.8 | 7.7 |
| | self-employed | 7.5 | 7.2 | 7.0 | 5.5 | 4.6 | 4.6 |
| | retired | 16.0 | 15.2 | 17.2 | 26.8 | 31.4 | 32.0 |
| | non labour market | 16.7 | 22.7 | 22.2 | 13.1 | 12.9 | 9.6 |
| Ethnical group | non-Hisp. Caucasian | 70.0 | 72.7 | 72.6 | | | |
| | non-Hisp.African A. | 11.2 | 11.7 | 11.6 | | | |
| | non-Hisp. Other | 6.1 | 4.8 | 4.6 | | | |
| | Latinos and Latinas | 12.7 | 10.8 | 11.2 | | | |
| Region of living | Northeast | 18.9 | 19.0 | 18.7 | | | |
| | Midwest | 22.4 | 22.9 | 22.9 | | | |
| | South | 35.9 | 35.4 | 35.8 | | | |
| | West | 22.7 | 22.7 | 22.6 | | | |
| | West-Germany | | | | 81.7 | 80.7 | 76.9 |
| | East-Germany | | | | 18.3 | 19.3 | 23.1 |
| Nationality | Germans | | | | 92.9 | 92.9 | 94.9 |
| | Non-Germans | | | | 7.1 | 7.1 | 5.1 |

a representative nation panel via the Internet[3]. The data collection was supported by TESS (Time-sharing Experiments for the Social Sciences)[4] that is run by the University of Michigan at Ann Arbor and Ohio State University and funded in part by the National

[3]Uslaner (2004) shows that people using the internet regularly are not more trusting than those who rarely use it. In our regression we control for the frequency people use the internet. Since it does not affect trusting behaviour we are confident that this methodological difference does not change our results.

[4]For further information on TESS, see http://www.experimentcentral.org or Mutz and Lupia (2003)

Science Foundation. A randomly drawn subsample of 1,489 participated in our study[5]. A few days after the fieldwork period the subjects got feedback and a check for the amount earned was sent to them by mail. Similar to the German sample, people are experienced panel respondents and received compensation for their participation before. So they know that the survey organisation is not going to cheat on them and that they will receive the money earned. The KN sample was recruited off-line through list-assisted random-digit dialling techniques. Once contacted, the sample members are provided with WebTV set-top boxes, including a remote keyboard and a pointing device, and free monthly Internet access (the connection is paid by KN) as an incentive to participate in the surveys. As a result of this two-step procedure, the KN sample is representative for the basic demographic characteristics of the U.S. population (Huggins and Krotki, 2001; Lee, 2006). As Table 3.1 documents, the weighted[6] distribution of our sample in the KN panel closely follows the distribution of the Current Population Survey (CPS) from April 2005.

Thus, our sampling procedures yielded representative samples of the German and U.S. population for a wide range of socio-economic variables. Furthermore, we would like to know if the sampling procedure selected more trusting people into the study since this is our variable of interest. We can assess this question indirectly by comparing the unweighted distribution of trust with the weighted one. If less trusting people are underrepresented in the survey we would expect the weighted mean to be lower than the non-weighted mean. We find that the differences in the means are very small and the weighted means (Germany: 5.62; United States: 6.77) lie within the 95% confidence intervals of the unweighted means (Germany: 5.44 – 5.86; United States: 6.57 – 7.02). For Germany, we have further evidence that selection into the experiment is not due to trust. People who drop out of the surveys in the following two years, did not exhibit a different level of trust in the experiment than those who stayed in the survey (t-test; $t = 0.26$, $p > .790$, two-sided)

In the following analysis the descriptive results are always weighted results and regression models are estimated with unweighted data. When analysing survey results, we use all data available. That is, in Germany we use the whole SOEP population of 24,000 individuals and in the United States all 1,489 participants in our study.

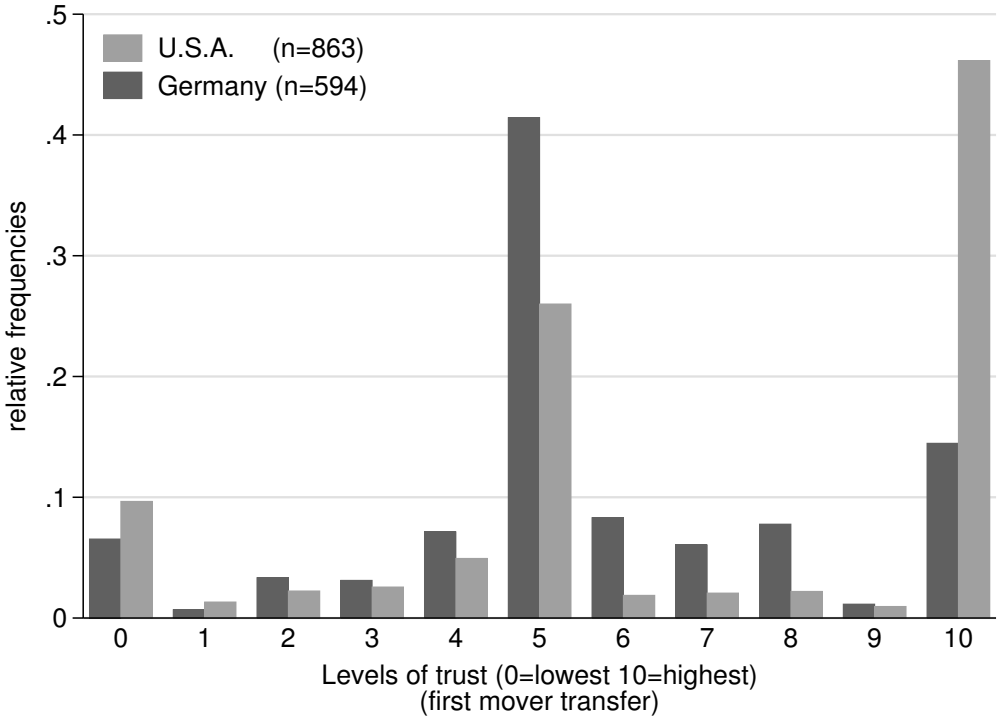
[5]Only one adult (age > 18) panel member per household was eligible for the survey and the study was conducted between April and May 2005.

[6]The post-stratification weights are based on gender, age, ethnicity, geographical distribution and education of subjects.

## 3.3 The Trust Gaps

In the United States, on average first-movers transferred 68% of their endowment to the trustee, whereas in Germany only 56% was transferred (Figure 3.1). Thus the average level of trust as measured by our experiment in the United States is 20% higher than in Germany (t-test; $t = 5.77$, $p < .001$, two-sided). In Germany, about 7% of the first-movers transferred nothing, 41% transferred five and 14% transferred the maximum. In the United States on the other hand, the peak of the distribution is not at half of the endowment but on the maximum level of trust. 46% of the first-movers transferred ten, 26% transferred five and 10% transferred nothing. Thus U.S. residents are more trusting of strangers than German residents. Interestingly, the low-trusting people in Germany (below the median person[7]) transferred about the same as the low-trusting U.S. residents (38% versus 36%). The difference in the mean is driven by differences in the upper part of the distribution. High-trusting people in Germany (above the median person) transferred on average 74% whereas high-trusting U.S. residents transferred on average 99% of their endowment. This difference is mirrored in the higher variance in the U.S. compared to the German distributions of levels of trust (Levene's test for equal variance; $W = 161.51$, $p < .001$). Concerning within country differences, we find that

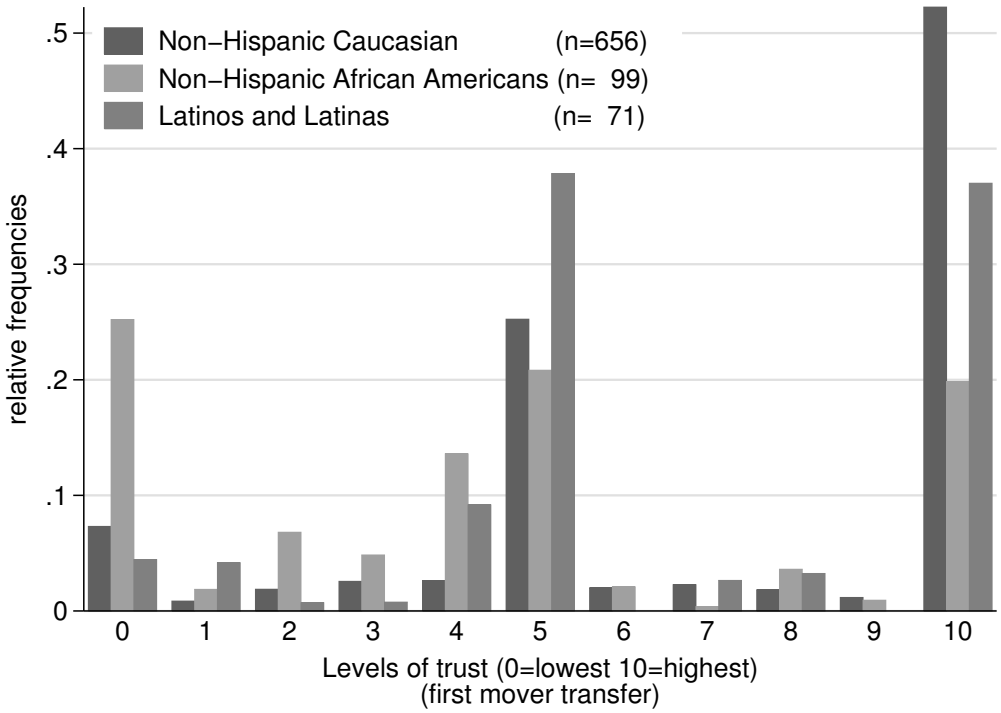
Figure 3.1: Levels of trust in the United States and Germany




[7]The median person transfers five in Germany and seven in the United States.

the average level of trust differs substantially between ethnic groups within the United States. The Caucasian populace transfers on average 72%, the Latino populace 65% and the African Americans 44% of their endowment to the trustee (Figure 3.2). The average trust level of Caucasian Americans is thus 65% higher than the level of trust of African Americans (t-test; $t = 6.01$, $p < .001$, two-sided) and that of Latinos and Latinas is 48% higher (t-test; $t = 3.29$, $p < .002$, two-sided) than that of African Americans. The average transfers of Caucasians and Latinos and Latinas are not significantly different. Furthermore we do not find any significant differences between the Northeast, Midwest, South and West of the United States[8] and between East and West Germany.

Figure 3.2: Levels of trust for non-Hispanic Caucasians, non-Hispanic African Americans and Latinos and Latinas




In the following we would like to analyse the gap in trusting behaviour between Germany and the United States and between the ethnic groups. Two conditions have to be fulfilled to be able to explain these observed trust gaps. First, we have to identify the factors that can explain a significant part of the variation in trusting behaviour. Second, these variables must exhibit different means for the two countries or groups of people. In the following we therefore focus on the determinants of trusting behaviour.

[8]We find a weakly significant difference between the Midwest (72% of the endowment) and the South (65% of the endowment) (t-test; $t = 1.76$, $p < .079$)

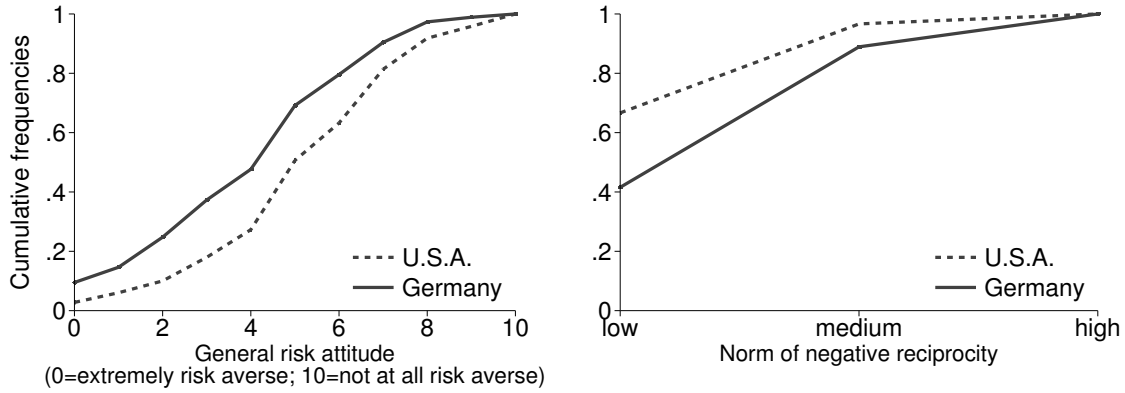
## 3.4 Possible Explanations for the Germany – United States Trust Gap

Economic theory predicts preferences and expectations/beliefs to influence behaviour. Concerning preferences we will on the one hand focus on risk preferences since trusting behaviour is a risky decision (Coleman, 1990) and on the other hand on social preferences, since social relations are involved in the trust game (Bohnet and Zeckhauser, 2004). Concerning beliefs the first-mover may form an expectation on how the second-mover will react to his/her transfer. Thus different expectations might lead to different behaviour. Furthermore, demographic characteristics possibly influence trusting behaviour.

### 3.4.1 Risk Preferences

The decision to trust is by definition a risky decision if some trustees are expected to have other-regarding preferences. The risk is such that the truster gains money if the trustee fully reciprocates the trust placed in him/her and the truster loses money if the trustee behaves selfishly. Furthermore, the risk is increasing with the level of trust. That is, the more a truster trusts the larger is the difference between the worst and best outcome. Thus, a risk averse first-mover is expected to trust less than a non-risk averse first-mover. We measured player's risk preferences by asking them to what extent they are willing to take a risk in general. They indicated their answer on an eleven-point Likert scale, with zero indicating completely unwilling to take risks and ten indicating completely willing to take risks. It has been shown that this question is correlated with actual risk taking behaviour, as for example lottery choices, investments in stocks, smoking, self-employment and participating in medical studies (Dohmen *et al.*, forthcoming; Roe *et al.*, 2009). To the best of our knowledge, this is the first study that compares individual risk preferences across countries on a representative basis. We find that in the risk question on average people in the United States report less risk aversion than people living in Germany (Mann-Whitney U-test; $z = 17.67$, $p < .001$, two-sided). This can be seen in Figure 3.3 where the cumulative distribution of our measure of risk preferences is depicted. That people in the United States are less risk averse compared to Germans can be seen in that the curve representing the U.S. residents (dashed curve) lies to the right of the curve representing the German residents (solid curve). This result is in line with previous arguments and observations. From a historical point of view it is argued that Europeans that migrated to the United States (the largest group of immigrants came from Germany) were those who were less risk averse (Alesina *et al.*, 2001). Due to intergenerational transmission of preferences, this can explain why U.S. Americans are less risk averse than Germans. A behavioural observation in line with our

Figure 3.3: Risk- and betrayal aversion in Germany and the United States




result is the fact that the proportion of U.S. American households that invest in risky assets is double that of households in Germany (Guiso, 2002). Our finding is also not restricted to our specific measure of risk aversion. An alternative survey measurement is used by Barsky *et al.* (1997) who measured risk preferences using hypothetical situations about people's willingness to give up a secure income for an insecure, on average higher income. In an accompanying unpublished study to the SOEP we used the same other measurement and find again that Germans are more risk averse than U.S. residents in the study by Barsky *et al.* (1997). This reinforces our result that U.S. residents are less risk averse than Germans. The next step will be to see if risk preferences indeed influence the decision to trust as predicted by economic theory. The previous empirical evidence in non-representative samples (mostly students) generally supports this prediction (Andreoni *et al.*, 2003; Ashraf *et al.*, 2003; Eckel and Wilson, 2000, 2004; Sapienza *et al.*, 2007; Schechter, 2007; Snijders and Keren, 1999).

### 3.4.2 Social Preferences

Theories of social preferences (e.g. Falk and Fischbacher, 2006; Fehr and Schmidt, 1999) assume that some people experience a disutility solely from unequal payoffs or getting betrayed. By not trusting at all, a first-mover can avoid getting betrayed and can exclude disadvantageous inequality between him/her and the trustee. The more a truster trusts, the higher is the potential inequality and the stronger is the potential betrayal. Thus, these models predict that an inequality or betrayal averse person trusts less than one that does not mind inequity or to get betrayed. The reason is that by trusting less, the first-mover is less vulnerable to experience inequality or betrayal. As predicted by these models, it has been shown that in non-representative trust games first-movers incur betrayal costs, costs shown to be more than purely monetary losses (Bohnet and

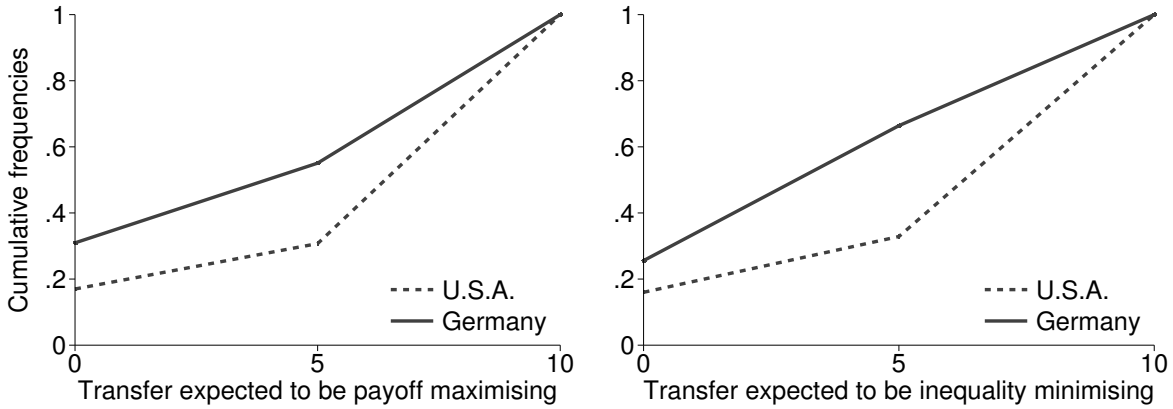
Zeckhauser, 2004; Bohnet *et al.*, 2008). Furthermore, they show that betrayal costs cannot be explained by risk preferences. Thus, we predict that the more betrayal averse a first-mover is, the less he/she trusts. We measure betrayal aversion by two survey questions on how much a person agrees with a norm of negative reciprocity. People indicated on a Likert scale how much they agreed with the statements 'If I suffer a serious wrong, I will take my revenge as soon as possible, no matter what the costs.' and 'If someone offends me, I will also offend him/her.' We categorised the answers into the three categories low, medium and high. These questions are taken from Perugini *et al.* (2003), in which it was argued that people high on the scale are sensitive to negative interpersonal behaviour. We find that people from the United States are less betrayal averse than people from Germany (Mann-Whitney U-test; $z = 19.78$, $p < .001$, two-sided) (Figure 3.3).

### 3.4.3 Expectations

In the existing literature on trust games there is no doubt, that expectations strongly influence trusting behaviour (Ashraf *et al.*, 2006; Chaudhuri and Gangadharan, 2007; Garbarino and Slonim, 2009; Vyrastekova and Garikipati, 2005). Although this sounds almost trivial, only few among the many articles on trust games actually used expectations as an explanation for trusting behaviour. One difficulty is that expectations should be elicited for the whole strategy space. The reason is best explained with an example. Imagine a situation with two first-movers. We know that both expect the second-movers to transfer back five points after they transfer five points. Further we observe that one of these two first-movers actually transfers five points and the other transfers ten points. In this case expectations have no explanatory power for the different transfers of the two first-movers. The solution could well be that the first-mover who transfers ten points has more optimistic expectations about the behaviour of the second-movers if he transfers ten points than has the first-mover who actually transfers five points. Thus we asked people 'What would you expect participant 2 to do in the following three examples?' These examples were if they transferred zero, five or ten points. We therefore have expectations concerning the behaviour of the second-mover at three different points distributed over the whole strategy space. By soliciting the first-movers' expectations at zero, five and ten points we take major differences in first-movers' expectations into account and are able to analyse in depth the role of expectations in trusting behaviour. Given the expectations at zero, five and ten points, we are able to calculate which of the three transfers is believed to yield the highest payoff. We thus have a measure of what first-movers believe to be the payoff-maximising transfer. If self-interest is a driving motive of trusting behaviour, trusters' transfers are likely to increase with

the expected payoff-maximising transfer. The relationship between first-mover transfer and payoff-maximising transfer would be weaker for individuals with social preferences. Similarly, we compute which of the three transfers is believed to produce the smallest income inequality between the first and the second-mover. We thus know what the first-movers believe to be the inequality-minimising transfer. If first-movers' transfers are also motivated by a concern for fairness and equality, then truster's transfers are likely to increase with the expected inequality-minimising transfer. This relationship would be stronger for individuals with a strong social preference. We find that U.S. residents have significantly more optimistic expectations concerning the behaviour of the trustee. The transfer expected to be payoff-maximising is significantly higher in the United States than in Germany (t-test; $t = 6.81$, $p < .001$, two-sided). In more detail (Figure 3.4), 69% of U.S. first-movers expect that to place trust fully is the most profitable strategy, whereas only 45% of the Germans are of this opinion. Similarly, the transfers expected to minimise the income inequality are on average much higher in the United States than in Germany (t-test; $t = 8.11$, $p < .001$, two-sided). Only 34% of Germans believe that a transfer of ten is the best when concerned with inequality, whereas this share is double (67%) for U.S. Americans (Figure 3.4). The question arises whether these expectations

Figure 3.4: Transfer expected to be payoff-maximising and inequality-minimising




are correct in the sense that they reflect the observed behaviour of second-movers. We did not ask people what they expect the 'average' second-mover to do, because people have difficulties in understanding what an average person is. Our question was framed for the most likely reaction of the second-movers. To assess whether first-movers correctly predict the behaviour of second-movers we compute how many first-movers expected a back-transfer that was among the most likely observed back-transfers. Overall the observed back-transfer of second-movers was remarkably well predicted. In the United States 77% of first-movers predicted a back-transfer that was among the most likely

observed back-transfers and in Germany this share was 67%.

### 3.4.4 Demographic Characteristics

An advantage of using a representative sample is that the variations in demographic factors in modern societies are much larger compared to the student samples common in the experimental literature. It is therefore not surprising that income is seldom included in the analysis of trusting behaviour. We have extensive information on the income situation of the subjects. We use the total income before taxes of the household the person is living in as a measure of the income situation. The size of the household is used to correct for how much of the income is disposable for each member of the household. We thereby take into account that often not every adult member of a household has a personal income and that households with a lot of children need a higher income to have the same economic status as other households with no children. In order to make the incomes comparable across countries we used the purchasing power parity exchange rate between the Euro and the US$ in 2005, from the OECD (1 US$ = 0.883€). The median yearly household income in the United States is around 40,000 US$ whereas in Germany it is 32,500 US$. Previous research using questionnaires suggest a positive relation between income and the level of trust (e.g. Alesina and La Ferrara, 2002; Bellemare and Kroeger, 2007). We therefore expect income to positively influence trusting behaviour. We further expect age to influence trusting behaviour based on the evidence by Bellemare and Kroeger (2007) and Sutter and Kocher (2007), who find an inverted-U relation between age and trusting behaviour. A further interesting characteristic of people is their religious affiliation and religiosity (Iannaccone, 1998). The results from previous research on the relation of trust and religion is mixed. Using the World Values Survey, Guiso *et al.* (2003) find for a total of 66 countries that the more religious people are the more they trust and that Christian religions foster trust, but less so for Catholics than for Protestants. Welch *et al.* (2004) on the other hand do not find this relation for people in the United States. We measure religiosity by the frequency of going to church and/or visiting religious events.

### 3.4.5 Control Variables

Altruistic motives could possibly increase trusting behaviour. However, transfers that are only based on altruistic motives are not compatible with our concept of trust. Our concept requires that the incentive to trust is based on the potential gain. The incentive for purely altruistic people is to transfer the whole endowment and this incentive is clearly not based on the potential gain. Therefore, we control for altruistic motives in

the main regressions in order to make sure that our results are not driven by differences in altruism. We use the amount of time people spend in volunteering in clubs and social services as a proxy measure for the degree of altruism. People who volunteer are ready to help others and spend time for others and thus behave in a selfless manner. Respondents could answer the question by indicating that they engage in volunteering weekly, monthly, seldom or never. This measure of altruism is correlated with other measures such as blood donation or charitable giving (Piliavin and Charng, 1990) and captures the essence of altruism. To be certain that we capture altruism with the variable volunteering and not purely sociability we included sociability as a control as well. We measure sociability with the frequency of meeting friends and neighbours.

## 3.5 Determinants of Trusting Behaviour

We argued that risk aversion, social preferences, and first-movers' expectations about the reaction of second-movers are determinants of trusting behaviour. Further we showed that these potential explanations exhibit different means in the two countries. It remains to check whether our conjectures about the determinants of trusting behaviour hold true. Estimating a linear model by ordinary least squares (OLS)[9] in which the trusting behaviour of first movers is regressed onto the different variables can test this. Our results show that the three conjectures cannot be rejected at conventional significance levels (regression 2 – 4 in Table 3.2). The most risk averse people trust 1 point less on average on the scale from zero to ten than the least-risk averse. The most betrayal averse people transfer on average 1.2 points less than people who are the least betrayal averse. Controlling for the transfer expected to be payoff-maximising, a person who thinks a transfer of ten points is payoff-maximising transfers on average 0.9 points more than a person who thinks it is a transfer of zero points. Similarly, trust increases by 2.3 points if the transfer expected to be inequality-minimising increases by ten points.

Further, we hypothesised that for people with social preferences the relation between trusting behaviour and the transfer expected to be profit maximising is weaker and the relation with the transfer expected to be inequality minimising stronger. In order to address this question, we create an overall measure for social preferences. We have two measures of social preferences that are betrayal aversion and altruism. We combine these two measures into a dummy variable if a person has social preferences or not. This variable is one if a person frequently volunteers and at the same time scores high on betrayal aversion and zero if the opposite is true. People who only score high or low in one of the two measures are left unclassified. We then regress trusting behaviour on the

[9]Estimating the model by ordered probit or tobit does not change the results

dummy of being a U.S. resident, the two measures of expectations and their interaction with the dummy variable for social preferences and the dummy itself. We find that both interaction terms are significant at the 5% level and have the expected sign. For the transfer expected to be profit maximising we find that the coefficient for people with low social preferences is significantly positive ($\hat{\beta}$ = 0.11 t = 3.56 p < .001) whereas for people with strong social preferences it is significantly lower (Interaction term: $\hat{\beta}$ = -0.22 t = 2.92 p < .004). However the negative effect is not significantly different from zero (F[1,810] = 2.08 p > .149). Concerning the relation of the transfer expected to be inequality minimising with trusting behaviour; we find that it is almost double for people with strong social preferences (Interaction term: $\hat{\beta}$ = 0.19 t = 2.00 p < .047) than for people with weak social preferences ($\hat{\beta}$ = 0.21 t = 5.99 p < .001). The finding that the influence of our measure of expectations varies with the degree of social preferences nicely supports the validity of our measures for expectations.

Table 3.2: Determinants of trusting behaviour (first-mover transfer)

| Dependent variable: first-mover transfer | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Dummy of being | 1.18** | 0.99** | 1.01** | 0.48** | 0.14 | 0.06 |
| an U.S. resident | (0.16) | (0.17) | (0.17) | (0.16) | (0.18) | (0.20) |
| Risk aversion: high | | −1.00** | | | −0.93** | −0.67** |
| (Base: low) | | (0.23) | | | (0.22) | (0.22) |
| Risk aversion: medium | | −0.62** | | | −0.57** | −0.50** |
| | | (0.20) | | | (0.18) | (0.18) |
| Betrayal aversion: med. | | | −0.42* | | −0.44** | −0.30° |
| (Base: low) | | | (0.18) | | (0.17) | (0.17) |
| Betrayal aversion: high | | | −1.20** | | −1.27** | −1.03** |
| | | | (0.32) | | (0.32) | (0.32) |
| Transfer expected to be | | | | 0.09** | 0.09** | 0.08** |
| payoff-maximising | | | | (0.02) | (0.02) | (0.02) |
| Transfer expected to be | | | | 0.23** | 0.23** | 0.21** |
| inequality-minimising | | | | (0.02) | (0.02) | (0.02) |
| Constant | 5.64** | 6.29** | 5.96** | 3.89** | 4.85** | 3.87** |
| | (0.11) | (0.19) | (0.15) | (0.16) | (0.26) | (0.50) |
| Observations | 1,436 | 1,436 | 1,436 | 1,436 | 1,436 | 1,436 |
| Adjusted-$R^2$ | 0.03 | 0.05 | 0.04 | 0.16 | 0.18 | 0.21 |
| Demographics included | no | no | no | no | no | yes |

Notes. OLS regression, coefficients and standard errors (in parentheses) reported. °, *, ** denote statistical significance on the 10%, 5% and 1% level, respectively.

The stability of each of these determinants of trusting behaviour can be verified by simultaneously controlling for the other variables. The results show that these variables each have an influence on trust independently from each other (regression 5 in Table

3.2). The coefficients are very similar to the regression without controlling for the other determinants and all coefficients remain significantly different from zero. Even to control for demographic and other control variables does not weaken the significance of these coefficients in an important way (regression 6 in Table 3.2).

The unexplained difference in the trust level is captured in the cited regressions by the dummy-variable of living in the United States or Germany. When controlling for the three important determinants, the demographic and control variables, the size of the unexplained difference in the trust level is reduced from 1.18 US$ to essentially zero (regression 1 and 6 in Table 3.2). Most of the reduction is due to the inclusion of the three determinants of trusting behaviour, as the coefficient in column 5 is already not significantly different from zero. This suggests that our variables – lower risk aversion, a lower level of betrayal aversion, and more optimistic expectations about the behaviour of the trustee in the United States compared to Germany – fully explain the trust gap.

This indicates the importance of our determinants as discussed above. Among the demographic variables (regression 2 in Table 3.3), a person's income and whether the person is living in a detached or semi-detached home have a positive influence on trusting behaviour. People with a household income in the highest quartile transfer on average 1.14 points more than people with an income from the lowest quartile. Whether the person is living in a detached or semi-detached home can be interpreted as a proxy for wealth, which suggests that not only people with a high income trust more but also wealthy individuals trust more than relatively poor people. Other demographic variables such as gender, age, education and employment status do not influence trusting behaviour. The control variable altruism is far from being significant and does not affect the overall conclusion of our analysis. Being more sociable with friends, neighbours and relatives has a marginally significant, positive effect on trust. That is, the more sociable someone is the more trust this person has in strangers. Frequent usage of the Internet does not affect our results either.

As found in previous research, we find an inverse u-shaped relation between trust and age when not including other variables. Regressing age and age$^2$ and a dummy for living in the United States onto trusting behaviour yields a significant and positive coefficient on age ($\hat{\beta}$ = 0.06 p < .02) and a significant negative coefficient for age$^2$ ($\hat{\beta}$ = -0.001 p < .01). As soon as we control for either income or education the relation vanishes. This suggests that the age-effects in previous studies could be partly driven by different income or education of young and elderly. A similar result is found for the religiosity of people. A regression of a dummy variable indicating whether people go to church and a dummy for people living in the United States on trusting behaviour shows that people visiting religious events exhibit higher levels of trust than people never visiting a religious

event ($\hat{\beta}^{religiosity} = 0.44$ p $< .02$). However, the effect is reduced and not significant any more as we include either our measure of altruism – the frequency of volunteering in clubs and social services – ($\hat{\beta}^{religiosity} = 0.25$ p $> .20$) or income ($\hat{\beta}^{religiosity} = 0.27$ p $> .13$). Sociability is not the driving force behind the effect of altruism. Only controlling for sociability does not render the effect of religiosity ($\hat{\beta}^{religiosity} = 0.36$ p $< .05$). This suggests that the relation of religiosity and trust is driven by people's income and their degree of altruism. We also tested for an influence of religious affiliation and the political party people most identify with. Neither of them has a significant influence on trusting behaviour.

We give evidence that risk aversion, betrayal aversion, and expectations are determinants of trusting behaviour and that these determinants exhibit different means in the United States and Germany. Furthermore, these variables fully explain the trust gap.

## 3.6 Decomposition of the Germany – United States Trust Gap

It remains to show the relative importance of these determinants when explaining the trust gap between the United States and Germany. With this goal we apply a decomposition of the determinants for the trust gap similar to the Blinder-Oaxaca decomposition used to decompose wage gaps in labour economics (Blinder, 1973; Oaxaca, 1973). Based on the estimation of the model with OLS one can show (appendix 3.B) that the relative importance of an explanatory variable in explaining the trust gap is equal to the product of the regression coefficient and the absolute difference in the mean of that variable in Germany and the United States (columns 2 and 5 in Table 3.3). Thus the sum of these products is exactly equal to the revealed trust gap of 1.12 points (1.18 – 0.06). The bars in Figure 3.5 represent the relative importance of the determinants in explaining the trust gap.

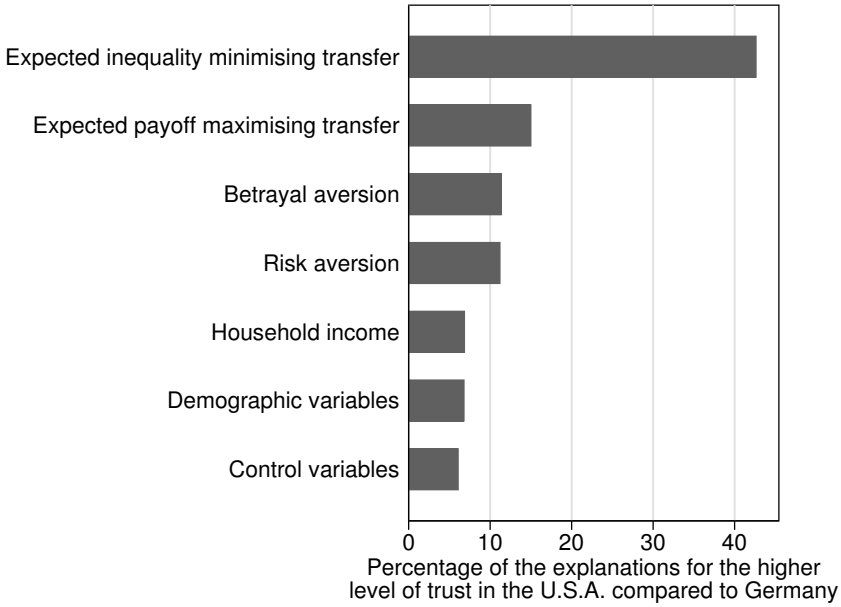
This decomposition shows that the most important determinant in explaining the trust gap between the United States and Germany are the expectations concerning the behaviour of the second-mover. The differences between the United States and Germany in the beliefs of which transfers minimise inequality explains 43% of the trust gap. The Germans are more pessimistic than the U.S. residents in their expectation of which transfer is payoff-maximising, and this difference explains another 15% of the trust gap. The lower level of betrayal aversion in the United States compared to Germany can explain another 11% of the trust gap. The fact that U.S. residents are more prepared to take risks than Germans explains 11%, while the lower incomes in Germany compared to the United States explains 7% of the trust gap. Only 7% and 6% of the trust

Table 3.3: Determinants of trusting behaviour (first-mover transfer)

| Dep. var.: first-mover's transfer | (1) | (2) | mean U.S.A. | mean Germany | (3)-(4) | (5)*(2) |
|---|---|---|---|---|---|---|
| Dummy of being an U.S. resident | 1.18** | 0.06 | 1.00 | 0.00 | 1.00 | 0.06 |
| | (0.16) | (0.20) | | | | |
| Risk aversion: high (Base: low) | | −0.67** | 0.19 | 0.39 | -0.20 | 0.14 |
| | | (0.22) | | | | |
| Risk aversion: medium | | −0.50** | 0.45 | 0.43 | 0.02 | -0.01 |
| | | (0.18) | | | | |
| Betrayal aversion: medium (Base: low) | | −0.30° | 0.28 | 0.50 | -0.21 | 0.06 |
| | | (0.17) | | | | |
| Betrayal aversion: high | | −1.03** | 0.03 | 0.09 | -0.06 | 0.06 |
| | | (0.32) | | | | |
| Transfer expected to be payoff-maximising | | 0.08** | 7.58 | 5.60 | 1.98 | 0.17 |
| | | (0.02) | | | | |
| Transfer expected to be inequality-minimising | | 0.21** | 7.54 | 5.31 | 2.23 | 0.48 |
| | | (0.02) | | | | |
| Dummy of being a female | | −0.09 | 0.51 | 0.52 | -0.01 | 0.00 |
| | | (0.16) | | | | |
| Age: 18 - 30 (Base: 31 - 50) | | 0.11 | 0.21 | 0.12 | 0.09 | 0.01 |
| | | (0.25) | | | | |
| Age: 51 - 92 | | −0.09 | 0.45 | 0.50 | -0.06 | 0.01 |
| | | (0.21) | | | | |
| Household income: 2nd quartile | | 0.28 | 0.18 | 0.27 | -0.09 | -0.02 |
| (Base: 1st quartile) | | (0.24) | | | | |
| Household income: 3rd quartile | | 0.83** | 0.19 | 0.20 | -0.01 | -0.01 |
| | | (0.25) | | | | |
| Household income: 4th quartile | | 1.14** | 0.40 | 0.30 | 0.10 | 0.12 |
| | | (0.24) | | | | |
| Household size | | −0.10 | 2.71 | 2.65 | 0.06 | -0.01 |
| | | (0.07) | | | | |
| Head of household | | −0.22 | 0.78 | 0.56 | 0.21 | -0.05 |
| | | (0.18) | | | | |
| Lives in a detached or semi-detached home | | 0.31° | 0.73 | 0.50 | 0.23 | 0.07 |
| | | (0.17) | | | | |
| Education: high school | | 0.12 | 0.61 | 0.68 | -0.07 | -0.01 |
| (Base: less than high school) | | (0.23) | | | | |
| Education: more than high school | | 0.44 | 0.24 | 0.16 | 0.08 | 0.03 |
| | | (0.28) | | | | |
| Employment status: unemployed | | −0.21 | 0.06 | 0.05 | 0.00 | -0.00 |
| (Base: Employed) | | (0.34) | | | | |
| Employment status: self employed | | 0.23 | 0.08 | 0.05 | 0.03 | 0.01 |
| | | (0.31) | | | | |
| Employment status: retired | | 0.18 | 0.22 | 0.32 | -0.10 | -0.02 |
| | | (0.23) | | | | |
| Employment status: non labour market | | 0.33 | 0.19 | 0.12 | 0.07 | 0.02 |
| | | (0.24) | | | | |
| Daily internet user | | −0.06 | 0.47 | 0.13 | 0.34 | -0.02 |
| | | (0.18) | | | | |
| Dummy of behaving selflessly | | 0.18 | 0.69 | 0.34 | 0.35 | 0.06 |
| | | (0.17) | | | | |
| Dummy of behaving sociable | | 0.38° | 0.88 | 0.80 | 0.07 | 0.03 |
| | | (0.22) | | | | |
| Constant | 5.64** | 3.87** | | | | |
| | (0.11) | (0.50) | | | | |
| Observations | 1436 | 1436 | | | | |
| Adjusted-$R^2$ | 0.03 | 0.21 | | | | |

Notes. OLS regression, coefficients and standard errors (in parentheses) reported.
°, *, ** denote statistical significance on the 10%, 5 % and 1% level, respectively.

Figure 3.5: Decomposition of the trust gap between the United States and Germany



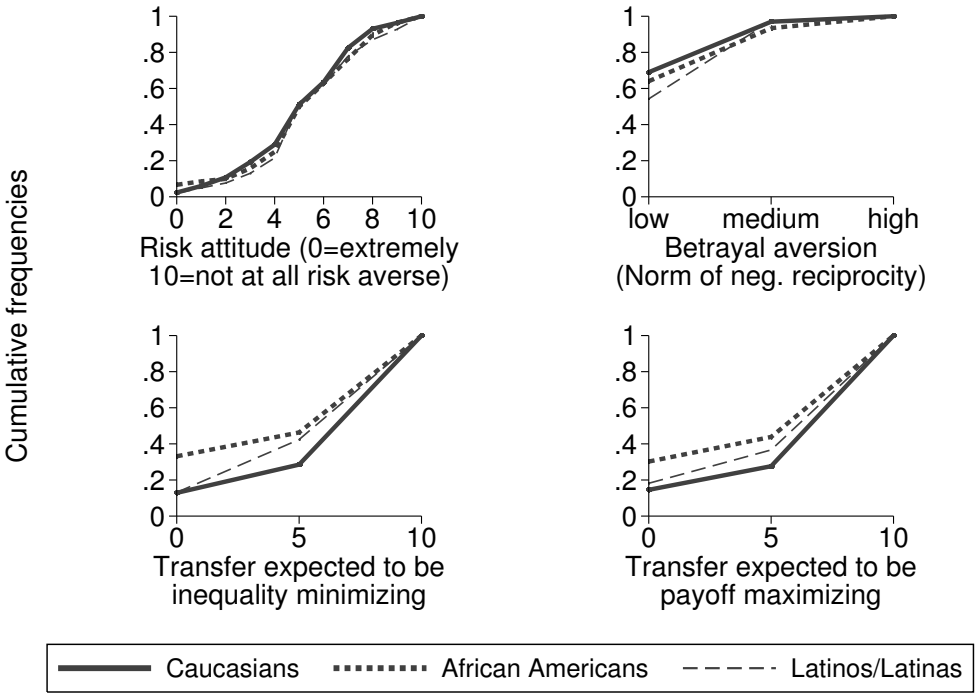

gap is explained by different demographic characteristics and different control variables, respectively.

## 3.7 Ethnic Trust Gap Within The United States

As noted earlier we find large differences in the level of trust between the different ethnic groups in the United States. People from the Caucasian populace exhibit the highest trusting behaviour among U.S. residents and they transfer on average 65% more than African Americans and Latinos and Latinas transfer on average 48% more than African Americans (Table 3.4: regression 1). We analyse this ethnic trust gap with the same explanatory variables as we used for the gap between Germany and the United States. The most important explanations for the Germany-United States trust gap are different expectations of the second-mover's behaviour, followed by risk aversion, betrayal aversion, income and other demographic variables. We find that African Americans have the least optimistic expectations regarding the behaviour of the second-mover followed by Latinos and Latinas and Caucasians (Figure 3.6). The differences between Caucasians and African Americans are highly significant for both the transfer expected to be inequality-minimising (t-test; $t = 3.06$, $p < .003$, two-sided) and the transfer expected to be payoff-maximising (t-test; $t = 2.64$, $p < .009$, two-sided). Although Caucasians have more optimistic expectations than Latinos/Latinas and the latter more optimistic than African Americans these differences are not significant. People from different ethnic

groups do not have different attitudes towards risk, but have significantly different social preferences. Interestingly, on average the Caucasian populace acts less by a norm of reciprocity than Latinos/Latinas (Mann-Whitney U-test; $z = 3.38$, $p < .001$, two-sided) and African Americans (Mann-Whitney U-test; $z = 1.98$, $p < .048$, two-sided). From the set of demographic variables the largest differences are unsurprisingly found in income. The median income of the Caucasian populace is between 40,000 to 50,000 US$ and thus substantially larger than the median income of Latinos/Latinas and African Americans which is between 25,000 to 30,000 US$. The differences between the Caucasians and the two other groups are highly significant (Median tests; both $p < .004$) whereas the median income for African Americans and Latinos/Latinas is not different.

Figure 3.6: Differences in the determinants of trust among non-Hispanic Caucasian, non-Hispanic African Americans and Latinos and Latinas




In short, we find that Caucasians have on average higher expectations concerning the transfers to be inequality-minimising and payoff-maximising, are less betrayal averse, and have a higher income compared to African Americans. We do not find a difference in risk aversion. We find no significant difference between the Latino populace and African Americans. The first condition, which is that potential explanations for the trust gap between the ethnic groups must exhibit different means for these groups, is fulfilled for the trust gap between Caucasian and African American, but not for the trust gap between Latinos/Latinas and African Americans.

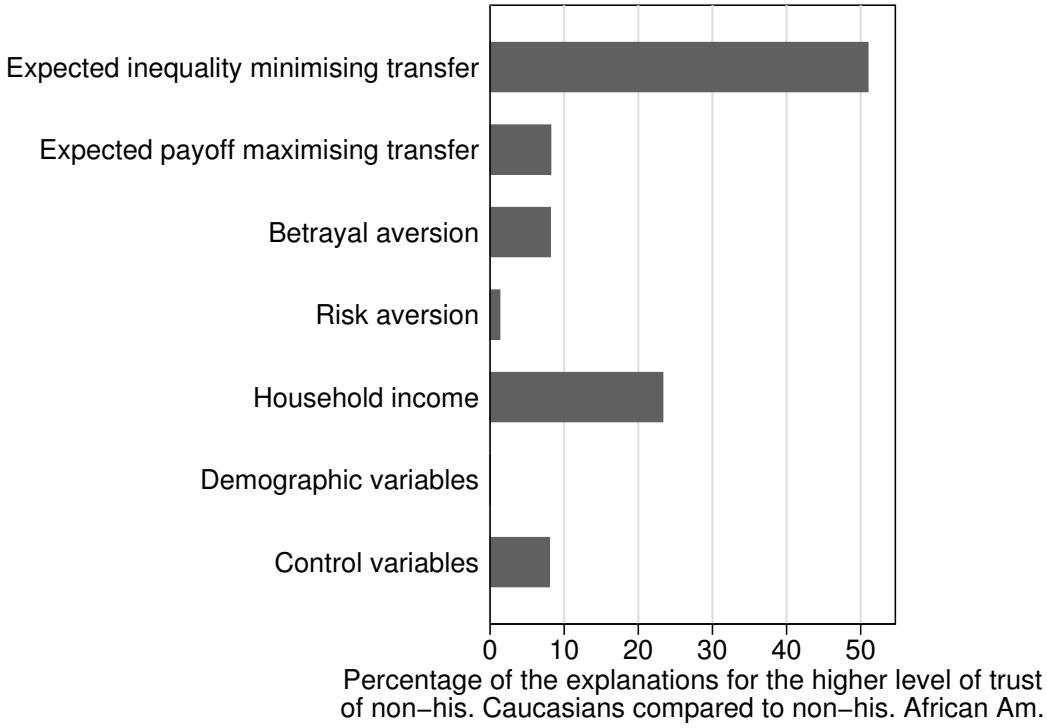
Table 3.4: Determinants of trusting behaviour within the United States

| Dep. var.: First-mover's transfer | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| Dummy for non-Hispanic Caucasians | 2.10** | 2.08** | 2.02** | 1.54** | 1.46** | 1.23** |
| (Base: non-Hispanic African American) | (0.38) | (0.38) | (0.38) | (0.39) | (0.37) | (0.38) |
| Dummy for non-Hispanic other | 1.71* | 1.66* | 1.67* | 1.28* | 1.20° | 0.97 |
| | (0.69) | (0.69) | (0.67) | (0.64) | (0.62) | (0.62) |
| Dummy for Latino or Latina | 1.65** | 1.58** | 1.73** | 1.39** | 1.39** | 1.46** |
| | (0.51) | (0.51) | (0.51) | (0.49) | (0.48) | (0.49) |
| Risk aversion: high (Base: low) | | −1.13** | | | −1.11** | −0.82* |
| | | (0.34) | | | (0.32) | (0.32) |
| Risk aversion: medium | | −0.82** | | | −0.68** | −0.55* |
| | | (0.24) | | | (0.22) | (0.23) |
| Betrayal aversion: medium | | | −0.78** | | −0.72** | −0.55* |
| (Base: low) | | | (0.26) | | (0.24) | (0.25) |
| Betrayal aversion: high | | | −1.58** | | −1.76** | −1.62* |
| | | | (0.59) | | (0.63) | (0.64) |
| Transfer expected to be | | | | 0.06° | 0.06° | 0.05 |
| payoff-maximising | | | | (0.03) | (0.03) | (0.03) |
| Transfer expected to be | | | | 0.29** | 0.29** | 0.27** |
| inequality-minimising | | | | (0.04) | (0.04) | (0.04) |
| Dummy of being a female | | | | | | −0.40° |
| | | | | | | (0.22) |
| Age: 18 - 30 (Base: 31 - 50) | | | | | | 0.05 |
| | | | | | | (0.32) |
| Age: 51 - 92 | | | | | | −0.31 |
| | | | | | | (0.29) |
| Household income: 2nd quartile | | | | | | 0.34 |
| (Base: 1st quartile) | | | | | | (0.32) |
| Household income: 3rd quartile | | | | | | 0.72* |
| | | | | | | (0.33) |
| Household income: 4th quartile | | | | | | 0.87* |
| | | | | | | (0.34) |
| Household size | | | | | | −0.12 |
| | | | | | | (0.09) |
| Head of household | | | | | | −0.25 |
| | | | | | | (0.30) |
| Lives in a detached or | | | | | | 0.52° |
| semi-detached home | | | | | | (0.27) |
| Education: high school | | | | | | −0.06 |
| (Base: less than high school) | | | | | | (0.32) |
| Education: more than high school | | | | | | 0.09 |
| | | | | | | (0.39) |
| Employment status: unemployed | | | | | | −0.44 |
| (Base: Employed) | | | | | | (0.47) |
| Employment status: self employed | | | | | | 0.10 |
| | | | | | | (0.38) |
| Employment status: retired | | | | | | 0.06 |
| | | | | | | (0.34) |
| Employment status: non labour market | | | | | | 0.11 |
| | | | | | | (0.30) |
| Dummy of behaving selflessly | | | | | | 0.19 |
| | | | | | | (0.25) |
| Dummy of behaving sociable | | | | | | 0.37 |
| | | | | | | (0.36) |
| Constant | 5.01** | 5.61** | 5.34** | 2.81** | 3.71** | 3.38** |
| | (0.36) | (0.38) | (0.37) | (0.41) | (0.45) | (0.73) |
| Observations | 856 | 856 | 856 | 856 | 856 | 856 |
| Adjusted-$R^2$ | 0.04 | 0.05 | 0.05 | 0.17 | 0.20 | 0.21 |

Notes. OLS regression, coefficients and standard errors (in parentheses) reported.
°, *, ** denote statistical significance on the 10%, 5% and 1% level, respectively.

A regression analysis shows that the determinants of trusting behaviour remain the same as discussed in the comparison between the United States and Germany (Table 3.4: regressions 2 – 5). Among the demographic variables, income and living in a detached or semi-detached home increase trusting behaviour (regression 6). Furthermore we find that women transfer 0.38 points less than men, which is significant at the 10% level. The dummy variables 'non-Hispanic Caucasians' and 'Latino or Latina' shows that these two groups show a significantly higher level of trust compared to African Americans. When controlling for all the variables discussed, the trust gap between Caucasian and African Americans is reduced by 41% from 2.10 to 1.23 points and the trust gap between Latinos/Latinas and African Americans by 12% from 1.65 to 1.46 points (Table 3.4: regression 1 and 6).

Figure 3.7: Decomposition of the trust gap between the non-Hispanic Caucasian and non-Hispanic African Americans



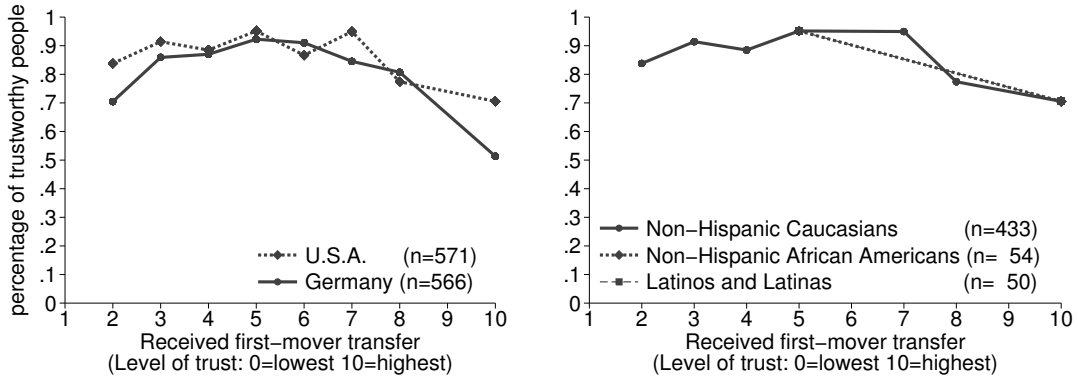

Once again we apply the method similar to the Blinder-Oaxaca decomposition in analysing the relative importance of the different explanations for the trust gap between Caucasian and African Americans. This decomposition confirms that the most important factor in explaining differences in trusting behaviour is the expectation of the reaction of the second-mover (Figure 3.7). Different expectations make up 59% of the explanation, which is very similar to the share of 58% in the explanation for the trust gap between the United States and Germany. The transfer expected to be inequality-

minimising represents 51% and the transfer expected to be payoff-maximising amounts to 8% of the explanation. The relative importance of the former is a bit higher in this case than in comparing the United States and Germany. The large differences in income represent 23% and a different betrayal aversion on average amounts to 8% of the explanation. The control variables explain another 8% and of minor importance in explaining the lower level of trust among African Americans compared to Caucasian Americans are risk aversion (1%), and other demographic variables (<1%).

## 3.8 Trustworthiness

We have shown that the trust gap between Germany and the United States is mainly explained through differing expectations regarding the behaviour of the second-mover. It is therefore interesting to analyse the behaviour of second-movers and compare this behaviour across nations and ethnicities. We say that a second-mover is trustworthy if his/her back-transfer makes the first-mover better off than the initial endowment of ten US$. In our design of the game this means that to be trustworthy a second-mover has to transfer back more than half of the received transfer of the first-mover. Controlling for the trust placed in them, we find that U.S. Americans are slightly trustworthier than Germans and that the degree of trustworthiness decreases with increasing levels of trust (Figure 3.8)[10]. A regression analysis confirms this graphical impression. U.S.

Figure 3.8: Trustworthiness




Americans are around 8 percentage points more likely to be trustworthy than Germans

[10]We control for the level of trust placed in them because a different level of trust was placed in the second-movers in the two countries. Remember that in the United States most people transferred ten points whereas in Germany five points was the most likely transfer. We did not plot the results for a first-mover transfer of one and nine since there were fewer than ten observations in each country. The average trustworthiness in Germany for a first-mover transfer of one is 90% and for nine it is 88%. In the United States the trustworthiness is 100% for a first-mover transfer of one point and 97% for a transfer of nine points.

are. The level of trustworthiness decreases with increasing trust. Between a first-mover transfer of 1 – 7 the level of trustworthiness is quite stable around 85% and then drops dramatically. It is 32 percentage points lower for a first-mover transfer of ten points compared to one of five points (regression 1 in Table V). Economic thinking suggests that the decision to behave trustworthily is influenced by social preferences. There is a positive and a negative side. On the one hand, altruistic people are expected to behave trustworthier than selfish people and, positively reciprocal people are expected to reciprocate and behave trustworthy when they perceive the decision of the first-mover as fair. On the other hand, negative-reciprocal second-movers might retaliate following a transfer perceived as unfair and thus are less likely to be trustworthy than non-negative-reciprocal second-movers.

Table 3.5: Determinants of trustworthiness (second-mover)

| Dependent variable: | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| dummy for behaving | **Received first-mover transfer ...** | | | | |
| trustworthy | all | $\geq 5$ | $\geq 5$ | $< 5$ | $< 5$ |
| Dummy of being an | 0.08** | 0.07* | 0.06° | 0.09 | 0.10 |
| U.S. resident | (0.03) | (0.03) | (0.03) | (0.05) | (0.06) |
| Positive reciprocity: medium | | | 0.06° | | |
| (Base: low) | | | (0.03) | | |
| Positive reciprocity: high | | | 0.08* | | |
| | | | (0.03) | | |
| Negative reciprocity: medium | | | | | −0.10° |
| (Base: low) | | | | | (0.06) |
| Negative reciprocity: high | | | | | −0.32* |
| | | | | | (0.17) |
| Dummy of behaving selflessly | | | 0.03 | | −0.07 |
| | | | (0.03) | | (0.06) |
| Dummy of being a | 0.01 | 0.01 | 0.00 | 0.04 | 0.01 |
| daily internet user | (0.03) | (0.03) | (0.03) | (0.06) | (0.06) |
| Controlled for received first-mover transfer | Yes | Yes | Yes | Yes | Yes |
| N | 1,129 | 960 | 960 | 169 | 169 |
| Pseudo-$R^2$ | 0.10 | 0.11 | 0.12 | 0.04 | 0.09 |
| Prob(dep.var = 1) | 0.81 | 0.81 | 0.81 | 0.85 | 0.85 |

Notes. Probit regression, marginal effects and standard errors (in parentheses) reported. °, *, ** denote statistical significance on the 10%, 5% and 1% level, respectively.

We measure altruistic motives and negative reciprocity in the same way as outlined in section 3.4. Positive reciprocity is measured as the agreement to the statement "I am willing to accept personal costs to help someone who helped me in the past." This

measure is based on Perugini *et al.* (2003), who show that the measures for positive and negative reciprocity are two distinct concepts. Since the reaction of second-movers can depend on whether people perceived first-movers' behaviour as trusting or distrusting, we analyse the determinants of trustworthiness for those who received less than 5 US$ and those who received 5 US$ or more separately[11].

Concerning the second-movers who received five or more points from the first-movers, we find that the positive-reciprocal persons are 8 percentage points more likely to be trustworthy. Our measure of altruism - the frequency of volunteering - has no influence on the level of trustworthiness (regression 3 in Table V). Taken together, these variables reduce the difference between U.S. Americans and Germans from 7 to 5 percentage points, where the latter is only marginally significant. The analysis for the second-movers who received less than five points is difficult because of the relatively low number of observations. We find that high-negative reciprocal persons are 32 percentage points less likely to be trustworthy than low-negative reciprocal persons (regression 5 in Table V). The other variables cannot explain the variance in trustworthy behaviour if the first-mover had a low level of trust. The different ethnic groups within the United States do not differ in this case. 82% of the Caucasian populace, 79% of African Americans and 78% of Latinas and Latinos are trustworthy. However, due to the small number of cases, these differences are not significant.

## 3.9 Conclusions

We have shown by means of two representative sample surveys, which were each combined with an experiment on trust, that U.S. residents place 20% more trust in strangers than do residents of Germany. This result in itself is of interest, since trust is widely regarded as an important lubricant of social interactions and economic growth. Our results might therefore indicate to some extent why we generally experienced better economic performance in the United States compared to Germany in the last two decades. Furthermore, and even more important, we have been able to elicit the reasons for this trust gap. We have shown that the gap can be completely explained through differing expectations, betrayal aversion, and risk preferences, in this order of effect size. U.S. residents are not per se more trusting people. Germans are not generally distrustful because they are German. However why are U.S. citizens more optimistic about the trustworthiness of strangers? And why do Germans shrink from risks and situations in which they might be betrayed? Clearly more research is necessary on this. Concerning trustworthiness we have shown that Americans are more likely to behave trustworthy

[11]Changing the cut-off value from five to six US$ changes the coefficient estimates only minimally

than Germans. The difference can only partially be explained by different preferences. A further observation might shed some light on this difference. We can check if the belief about others' trustworthiness is correlated with trustworthy behaviour. The survey measures of trust can be interpreted as a measure for the belief in others' trustworthiness, since it is highly correlated with the belief measures in the experiment (chapter 2). The belief in others' trustworthiness is indeed significantly correlated with the own trustworthy behaviour (Spearman rank correlation: $\rho = .07$, $p < .013$). It seems like the believed behaviour of others influences people's own behaviour. Overall, our data combining representative experimental results with extensive survey data allowed us to find and explain differences in trust on a level heretofore unique. Whereas trust is a topic of growing importance, previous studies tended to focus on small, selected, non-representative numbers of subjects. They can tell us much about the organic processes involved in trusting behaviour in individuals. Our own newly developed methodology on the other hand allows to capture and explain the heterogeneity of trust and its determinants, and thus illuminates the wider contexts by focusing on the differences between individuals on a large scale.

## Acknowledgments

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# 3.A Participants' Instructions

Welcome! You are participating in a study that consists of a short decision-making task. By completing this task, you have the chance to earn money. You are paired with another person who also makes a decision. The exact amount of money you will earn depends on your decision, as well as the of decision the person you are paired with. You will receive your money in the form of a check after the study is completed.

**Rules:**
There are two participants.
Each participant initially receives 10 points.
Each participant can use his/her 10 points in the following ways:

- he/she can either divide up the amount between him/herself and the other participant or
- keep the whole amount or
- transfer the whole amount to the other participant.

You will receive $1.00 for each point that you keep.

The other participant will receive $2.00 for each point that you transfer.

And vice versa:
For every point that the other participant transfers to you, you will receive $2.00.

## Further Instructions for First-movers

**You are Participant 1, so you make the first decision.**

You decide how many of your 10 points you keep and how many you want to transfer to the other person (Participant 2).

We inform Participant 2 of your decision without letting him/her know who you are.

Participant 2 therefore knows how many points you have transferred to him/her. Then he/she decides how many of his/her initial 10 points to transfer to you.

Both decisions (yours and that of Participant 2) determine the amount of money that you and Participant 2 earn. You will receive the sum of money in the form of a check.

## Further Instructions for Second-movers

**You are Participant 2, so you make the second decision.**

Participant 1 has already began and decided how many points to keep and how many to transfer to you. His/Her decision will be presented shortly.

Now it is your turn to decide how many of your 10 points you want to keep and how many you want to transfer to the other person (Participant 1).

Both decisions (yours and that of Participant 1) will determine the amount of money that you and Participant 1 will earn. You will receive the sum of money in the form of a check.

## Examples

Here are three examples:

- Each participant keeps his/her own 10 points and transfers 0 points to the other person. In this case, each participant receives $10.00.
- Each participant keeps 0 points and transfers all 10 points to the other person. In this case, each participant receives $20.00.
- Participant 1 keeps 3 points for himself/herself and transfers 7 points to Participant 2. We then tell Participant 2 that Participant 1 transferred 7 points. Participant 2 then decides to keep 4 points and transfers 6 points to Participant 1.

In this case, Participant 1 keeps 3 points and receives 2x6 points: 3+(2x6) = $15
Participant 2 keeps 4 points and receives 2x7 points: 4+(2x7) = $18.

## First-mover Decision

How do you want to divide up your **10 points**?
How many points (from 0 to 10) do you want to transfer to Participant 2 and how many do you want to keep?
*Enter an answer from 0 to 10 into each cell of the grid*

*I transfer . . . points*

*and keep . . . points*

## Second-mover Decision

**You are Participant 2;** Participant 1 already made his/her decision and transferred **X points** to you.
How do you want to divide up your **10 points**?

How many points (from 0 to 10) do you want to transfer to Participant 1 and how many do you want to keep?
*Enter an answer from 0 to 10 into each cell of the grid*

*I transfer . . . points*

*and keep . . . points*

## Expectations of First-movers

Now, we will tell Participant 2 how many points you transferred to him/her.

Participant 2 will then decide how many points he/she transfers to you.

"What would happen if. . . ?" This well-known, everyday question interests us, too. It is often important to predict how others will act. Therefore, we are asking you to answer an additional question:

What would you expect Participant 2 to do in the following three examples?

Enter an answer from 0 to 10 into each cell of the grid

- If I transferred 10 points to him/her, he/she would transfer ..... points to me.
- If I transferred 5 points to him/her, he/she would transfer ..... points to me.
- If I transferred 0 points to him/her, he/she would transfer ..... points to me.

## 3.B Derivation of the Decomposition

We estimate the following two equations by OLS:

$$trust = \alpha_0 + \delta_0 D^{USA} + \epsilon_0 \tag{B.1a}$$

$$trust = \alpha_1 + \delta_1 D^{USA} + X'\beta + \epsilon_1 \tag{B.1b}$$

*trust* is the number of points transferred in the experiment, $D^{USA}$ is the dummy variable being 1 if a person lives in the U.S.A. and being 0 if a person lives in Germany. $X$ is the matrix with all the other explanatory variables and $\epsilon$ is the error term. We are interested in the difference of the estimated coefficients of $D^{USA}$ in (1a) and (1b): $(\hat{\delta}_0 - \hat{\delta}_1)$
The estimated models (1a) and (1b) have the following properties:

$$\bar{trust} = \hat{\alpha}_0 + \hat{\delta}_0 \bar{D}^{USA} \tag{B.2a}$$

$$\bar{trust} = \hat{\alpha}_1 + \hat{\delta}_1 \bar{D}^{USA} + \bar{X}'\hat{\beta} \tag{B.2b}$$

Conditioning all the expectations on living in Germany ($D^{USA} = 0$) yields the following:

$$\bar{trust}^{Germany} = \hat{\alpha}_0 \tag{B.3a}$$

$$\bar{trust}^{Germany} = \hat{\alpha}_1 + \bar{X}'^{Germany}\hat{\beta} \tag{B.3b}$$

Setting (2a) and (2b) equal:

$$\begin{aligned}
\hat{\alpha}_0 + \hat{\delta}_0 \bar{D}^{USA} &= \hat{\alpha}_1 + \hat{\delta}_1 \bar{D}^{USA} + \bar{X}'\hat{\beta} \\
(\hat{\delta}_0 - \hat{\delta}_1)\bar{D}^{USA} &= \hat{\alpha}_1 - \hat{\alpha}_0 + \bar{X}'\hat{\beta} \\
(\hat{\delta}_0 - \hat{\delta}_1)\bar{D}^{USA} &= \hat{\alpha}_1 - \hat{\alpha}_0 + [\bar{D}^{USA}\bar{X}'^{USA} + (1 - \bar{D}^{USA})\bar{X}'^{Germany}]\hat{\beta}
\end{aligned}$$

$$(\hat{\delta}_0 - \hat{\delta}_1) = [\bar{X}'^{USA} - \bar{X}'^{Germany}]\hat{\beta} + [\bar{D}^{USA}]^{-1}[\hat{\alpha}_1 + \bar{X}'^{Germany}\hat{\beta} - \hat{\alpha}_0]$$

The last term is equal to the difference between 3a and 3b which is equal to zero. Therefore,

$$(\hat{\delta}_0 - \hat{\delta}_1) = \left[\bar{X}'^{USA} - \bar{X}'^{Germany}\right]\hat{\beta} \tag{B.4}$$

CHAPTER

# FOUR

# MISTRUST AND BETRAYAL: A VICIOUS CIRCLE

"The chief lesson I have learned in a long life is that the only way you can make a man trustworthy is to trust him; and the surest way to make him untrustworthy is to distrust him."

Henry L. Stimson (1867–1950), United States Secretary of State

## 4.1 Introduction

Whereas trust has been explored extensively across countries and societies, we still know little about the dynamic aspects of trusting behaviour in time. Trust in strangers as measured in surveys is reportedly in decline in the United States (Putnam, 2000). In 2006 trust as measured in the General Social Survey was at its lowest since 1972, remaining at that level in 2008. In other countries things look different. In Germany for example, trust as measured by the World Values Survey slightly increased from 1981 to 1999 and remained at the higher level in 2006. These two cases are only exemplary illustrations. They demonstrate that trust is not static in society but subject to dynamic change. However, with all these reported changes what remains open is the question of whether these changes are caused by cohort effects or are rather actual changes within the individual participants' level of trust. Whereas we know little about what drives aggregate levels of trust we know even less about the dynamic aspects of trust in individuals. Before we can adequately explore country-level dynamics of trust, we need to better understand what shapes trust on an individual level.

Related to trust on the individual level are also contributions to the public good in charitable giving, tolerance of foreigners, acceptance of large corporations and stock-trading behaviour (e.g. Brehm and Rahn, 1997; Dohmen *et al.*, 2008; Guiso *et al.*, 2008).

It is probable that trusting people are also more likely to engage with strangers and thus to agree to beneficial exchanges. Because of their importance for economic and social life, it is crucial to understand these changing levels of trust. What are the causes of decline? What breeds trust in individuals? Answers to these questions would have implications for economic and social policies, because understanding the dynamics of trust on the individual level would also have implications for changing levels of trust on the aggregate level.

Based on economic theory, I showed empirically in chapter 3 that trust is related to risk and social preferences and most importantly to expectations about others' trustworthiness. Among these three factors, expectations suggest themselves as a candidate variable that might explain changes in levels of trust. This is due to their adaptability: expectations change when people experience new situations or simply acquire more information. Thus, changing experiences in interactions with others can be assumed to explain changes in levels of trust. This reasoning is in line with social capital theory (Putnam, 2000). Putnam argues:

> *In America blacks express less social trust than whites, the financially distressed less than the financially comfortable, people in big cities less than small-town dwellers, and people who have been victims of a crime or been through a divorce less than those who have not had these experiences. It is reasonable to assume that in each case these patterns reflect actual experience rather than different psychic predisposition's to distrust. When such people tell pollsters that most people cannot be trusted, they are not hallucinating - they are merely reporting their experience.* (p.138)

In this paper we focus on experience as a factor by which levels of both trust and trustworthiness potentially change. This is not to say that there are no other factors involved. Nonetheless, experience seems to be central to all questions trust related and we focus on both sides of the coin: trusting and trustworthy behaviour. Thus we ask if positive or negative experiences change a person's trusting and trustworthy behaviour. We explore what the mechanisms are that lead to this change in behaviour. To be precise, we investigate if the connection between between experience and behaviour is indeed principally captured by changing beliefs or if there are other important factors involved.

To avoid confusion we must first clarify what we mean by the term 'trust'. In this, we follow James Coleman's concept of trust (Coleman, 1990). From his perspective, the following two points characterise the action of placing trust: First, trust implies that the truster freely transfers assets to another person, without controlling the actions of that other person or having the possibility to retaliate; second, there must be a potential

gain in order to have an incentive to trust. The incentive is such that if the other person is trustworthy, the truster is better off having trusted, and worse off if the other person does not merit the trust placed in him/her. 'Worse off' can be understood in an absolute sense where the truster ends up with less assets than initially. However it can also be understood relatively, where 'worse off' is seen in relation to the other person's assets. Note that in this concept, trust and trustworthiness are considered to be forms of reflected behaviour rather than personal characteristics or traits.

Despite the unquestioned importance of trust and the strong likelihood of it being closely related to experience, very few studies investigate if and how experience affects trust. Among these few none has established a causal relationship between experience and trusting *behaviour*. For self-reported trust as collected in surveys four recent papers are particularly worth mentioning as they focused on solving the causality issue. Olken (forthcoming) analysed the causal effect of the introduction of television and radio on trust in Indonesia. He finds that in regions with TV and radio reception people spend less time participating in the life of the community and their trust is lower when compared to regions without TV and radio reception. On the other hand, changing experiences can be observed in panel studies. Here, the Labonne and Chase (2008) paper is interesting. They studied the effect of the construction of roads on trust in the Philippines. Using a panel study they find that reduced transaction costs, captured by road construction, had a positive effect on trust in strangers but not on trust in other community members. Dekkers (2006) tested the social capital theory prediction that associational membership in organisations is a positive experience and therefore increases trust (Putnam, 1995). Although cross-sectional studies frequently find such relations, Dekkers (2006) finds no effect when analysing this relation with panel data. Analysing long-term effects of negative experience on trust, Nunn and Wantchekon (2009) show that people from sub-Saharan Africa whose ancestors were in danger of being caught for slavery, show lower trust in 2005 than people whose ancestors where not affected by slave trade. Whilst interesting, it must be kept in mind that such approaches can tell us only little about the intermediary factors relating long ago experiences with present day levels of trust.

Whereas the latter four studies measured trust with the help of surveys, to the best of our knowledge no study[1] has yet investigated the causal effect of experience on trusting *behaviour*. Moreover, there is also very little correlative evidence that experience affects trusting behaviour. As sole exception, we must mention Bohnet and Huck (2004), who find that in a repeated trust game the experience of trustworthiness in past rounds is correlated with trusting behaviour in the present round. However, it is not clear if this

[1]Harbring (2006) studied the causal effects of incentive schemes on trusting behaviour and concentrated on the effect of the scheme itself rather than the experience in those schemes.

effect is causal or not, since in the approach they used the trustworthiness a truster experiences in past rounds depends on his/her trusting behaviour in those past rounds. This constitutes a problem because past levels of trust are not only correlated with experience but also with present levels of trust. A correlation of experience with present levels of trust can thus not be disentangled from the correlation of present with past levels of trust. Unlike in their study, the experienced trustworthiness in our approach is independent of the individuals' previous actions. Thus, it is possible for us to explore the causal effects.

There is also only little empirical evidence for a correlation between experience and trustworthy behaviour. Bohnet and Huck (2004) find no relation of experience of trust with the propensity to behave trustworthily. This result does not allow a causal interpretation for the same reason as discussed above. If one interprets the revelation of the truster's expectation as an experience then the following two papers are also of interest. Ellingsen *et al.* (forthcoming) and Reuben *et al.* (forthcoming) analyse the effects of revealed first-mover expectations on trustworthy behaviour. While Ellingsen *et al.* (forthcoming) do not find any effect, Reuben *et al.* (forthcoming) do find a positive effect. The main difference in their design is that in the latter study the first-movers were informed that their expectation would be shown to the trustee, and in the former study the truster was not informed. This extra information could induce people to strategically manipulate their stated beliefs, thus explaining the different results.

There is slightly more evidence that experienced trust increases reciprocal behaviour measured by trustee's return ratio in trust games (Ben-Ner and Putterman, 2009; Ben-Ner *et al.*, 2007; Burks *et al.*, 2003; Chaudhuri and Gangadharan, 2007; Meidinger *et al.*, 1999)[2]. In contrast, several other studies did not find any correlation (Berg *et al.*, 1995; Csukas *et al.*, 2008; Willinger *et al.*, 2003). However, we would argue that one cannot use this data as evidence for a relation of trustworthiness and experienced trust. The reason is that we understand trustworthiness as a discrete either or behaviour. Someone either behaves in a trustworthy way or does not. There is no room in between[3]. Accordingly, if we analyse the second-mover decision as a zero/one decision, it follows that random second-mover behaviour would by design generate a negative relation between experienced trust and the probability of being trustworthy, the line between trustworthy and

[2]Croson and Buchan (1999) and Glaeser *et al.* (2000) also find a positive relation. However due to their slightly different design, the mere presence of fair types creates a positive relation between experienced trust and the return ratio. Croson and Buchan (1999) allowed second-movers to use their endowment for a back-transfer and Glaeser *et al.* (2000) did not give second-movers an endowment. Either change of the design has the effect that for fair types, the return ratio is higher, the higher the experienced trust.

[3]If the answer to the question if I can trust you, is 'a little', then my interpretation would be that you are not trustworthy and that I had better not trust you.

untrustworthy behaviour not lying in the middle of second-mover's strategy space. This explains why any result based on a cross-section analysis of second-mover behaviour in a trust game might be biased.

In sum, levels of trust that were self-reported in a survey have been shown to change with experience, that is, experience causally affects levels of trust. Concerning trusting behaviour, however, there are to the best of our knowledge no studies which analyse the causal effects of experience on trusting behaviour. In order to fill this gap we devised a procedure which embeds an experimental trust game in a panel survey study, thus combining the strengths of controlled behavioural measurement methods with both the ability of establishing causality through an exogenous experience and the large data coronary of a representative panel survey.

We measured trusting behaviour by means of an experiment - a modified trust game. In 2004 subjects played a trust game as first-movers. Their trust was either (i) abused, leaving them with less than than the trustee; (ii) fairly rewarded, meaning they earned the same as the trustee; or (iii) greatly rewarded, where they earned more than the trustee. We analyse how this exogenous experience changes people's level of trust one year later. We thus exclude effects of spontaneous and immediate emotional responses to the previous experience and therefore make it hard to find an effect. Nonetheless, we do find that a strong negative experience significantly reduces trusting behaviour. The main channels through which experience affects behaviour are the more pessimistic expectations about the counterpart's behaviour following a negative experience. The change in expectations can explain most of the influence which experience has on trust, but not its entirety. We further find that these expectations change most for people with low self-assurance. Moreover, once controlled for the change in expectations, people with strong social preferences reduce their trust much more after a negative experience than people with weak social preferences. Social preferences thus appear to be a moderator of the relationship between experiences and trust.

The propensity to behave trustworthily in 2005 is positively influenced by an increase from 2004 to 2005 in trust placed by the truster. Importantly, the effects of experiences on trust and trustworthiness are not only significant, but in most cases they are also larger in magnitude than other influencing factors, such as socio-economic characteristics and changes thereof.

We proceed with the description of the design of the study (section 4.2), followed by the results on trusting behaviour (section 4.3) and on trustworthy behaviour (section 4.4), and finally the conclusions.

## 4.2 Experimental Design

We measure trust and trustworthiness by using a trust game based on the design by Berg *et al.* (1995). There are two players, a first- and a second-mover. Both get an endowment of 10 Euros. The first-mover decides on how many of the 10 Euros he/she would like to transfer to the second-mover. Every Euro transferred is doubled by the experimenter. If the first-mover, for example, transfers 4 Euros, the second-mover receives 8 Euros. Then the second-mover decides on how much of his/her original endowment of 10 Euros he/she would like to send back. Again, every transfer is doubled by the experimenter. If both players are rational and selfish the subgame-perfect Nash equilibrium is not to send anything back as a second-mover and for the first-mover to transfer nothing. If the second-movers are for example inequity averse (Fehr and Schmidt, 1999) or reciprocal (Falk and Fischbacher, 2006), then their back-transfer might be positive and thus the first-movers have an incentive to transfer a positive amount even if they are completely selfish. If both players transfer the whole endowment to the other player then both earn more than their initial endowment. However, the first-mover decision is a risky decision, in which the outcome depends on the behaviour of the second-mover. That is why we call the first-mover decision a trusting decision, a subset of risky decisions. The more he/she transfers to the second-mover the more he/she trusts and thus becomes a truster. The second-mover on the other hand can behave trustworthily or not. We say that someone behaves trustworthily if their back-transfer is at least as high as what they received from the truster. With 'received transfer' we always refer to the amount transferred before it was doubled. The reasons why we chose this definition are twofold. On the one hand, the design of our trust game is completely symmetric and thus if the back-transfer is equal to the first-mover transfer, both players earn the same amount. On the other hand, this is actually the most likely outcome in the game. In 2005, 42% of trustees transferred the same amount back as they received, 33% transferred back less and 25% transferred back more than they received. Furthermore, when asked about what trusters expect back from the trustees in case they sent 0, 5 or 10 points, the modal expected back-transfer was 0, 5 and 10 points. Thus we think it is natural to assume that people perceive a back-transfer that is equal to their own transfer as trustworthy behaviour.

## Exogenous variation of trusters' experience

The exogenous variation of trusters' experience is created by playing the trust game twice[4]. The two games were played in spring 2004 and spring 2005. Thus, in the 2004 trust game trusters experienced if the trustees behaved trustworthily towards them or not. Since trusters are randomly matched with a new trustee in the next year, their experience is exogenous to their level of trust, and we analyse if the exogenous experience in 2004 changed their level of trust. If the second-mover was not trustworthy in 2004, we say the truster had a negative experience. If the trustee was just trustworthy, that is, he/she sent back the same amount as received, we say this is a neutral experience. In case the trustees were trustworthy and sent back more than received, we say this is a positive experience. The long time span between the two games prevents spontaneous and immediate emotional responses to the previous experience affecting behaviour. If we find a significant effect it would be a strong indicator that experience does indeed affect trusting behaviour.

In order to ensure that the experience is fully exogenous to behaviour, we restrict our analysis to those trusters who transferred half of their endowment to the trustee in 2004. All our subjects therefore have the same starting level of trust, eliminating any possibility of a correlation between experience and past trusting behaviour. This enables us to study how changes in their trust is affected by an exogenous manipulation of experience. In addition to preventing any endogeneity problems, this restriction also excludes the possibility that any effect might be driven by 'regression toward the mean' rather than a causal effect of experience. For any level of trust other than 5 points we cannot rule out that the results would be biased due to a 'regression toward the mean' effect. Thus, causal inference requires the restriction described above. The drawback of this restriction is that our sample is not a fully random sample anymore. We explore the existence of any observable biases below.

## Exogenous variation of trustees' experience

The exogenous variation of trustees' experiences is implemented in the following way. In 2004 and 2005 the trusters placed a certain level of trust into the trustees. Thus, the trust placed in a certain trustee either increases, decreases or is stable between the two years. We analyse if the change in the level of trust placed in trustees influences the probability that trustees behave trustworthily in 2005. In order to prevent a 'regression towards the mean' effect we restrict the analysis to trustees who received 5 points from

[4]Subjects actually played the trust game three times. We can only analyse the last two games because we did not elicit expectations in the first trust game.

the truster in 2005. If a second-mover decided randomly, the probability that this second-mover behaves trustworthily would decrease with higher levels of trust. This restriction is not necessary because our design is slightly different than the standard trust game where this would also be necessary[5]. This is also the reason why we analysed levels of trustworthiness in 2005 and not the change in trustworthiness over the two years.

The subjects of the experiments are a random sub-sample of the adult (age $> 17$) respondents in sample F of the German Socio-Economic Panel Study (SOEP). The SOEP is a household panel study representative of the German population (Wagner *et al.*, 2007). Sample F is the largest sample (n = 7,243) of the SOEP and was randomly drawn from the German population in 2000. 1,202 respondents participated in the experiment, and 594 were assigned to be first-movers and 608 to be second-movers. Among the second-movers 234 received 5 points in 2005 and were thus the trustees selected for our analysis. Note that since the amount of points trustees receive is independent of

Table 4.1: Socio-economic characteristics of the experimental sample

| | | Our sample 2005 first-mover | Our sample 2005 second-mover | SOEP 2005 |
|---|---|---|---|---|
| Women (in percent) | | 47.5 | 53.8 | 52.8 |
| Average age (in years) | | 53.9 | 51.0 | 49.1 |
| Average household income (in 1,000 Euros) | | 33.1 | 32.8 | 34.2 |
| Average household size (in persons) | | 2.6 | 2.6 | 2.7 |
| Education (in percent) | less than high school | 15.3 | 18.8 | 18.1 |
| | high school | 69.0 | 64.5 | 64.9 |
| | more than high school | 15.7 | 16.7 | 17.0 |
| Employment status | employed | 46.7 | 48.7 | 48.9 |
| (in percent) | self-employed | 4.1 | 6.4 | 5.7 |
| | unemployed | 6.2 | 7.7 | 5.9 |
| | non labour market/retired | 43.0 | 37.2 | 39.5 |
| Resident 1989 in | West Germany | 74.4 | 81.2 | 72.1 |
| (in percent) | East Germany | 20.2 | 13.2 | 22.6 |
| | Abroad | 5.4 | 5.6 | 5.4 |
| German nationality (in percent) | | 95.9 | 94.0 | 95.7 |
| Marital status (in percent) | Married | 69.2 | 61.5 | 59.4 |
| | Single | 15.7 | 19.7 | 23.9 |
| | Divorced/separated | 8.3 | 11.1 | 9.4 |
| | Widowed | 6.6 | 7.7 | 7.3 |
| Number of observations | | 242 | 234 | 7,243 |

their own actions and characteristics, the sample of trustees remains a randomly drawn

[5]Although it is possible to analyse how reciprocity depends on experienced trust but not the exact relation between trustworthiness and experienced trust.

sample of the SOEP. This is different for the first-movers. Among the first-movers 242 transferred half of their endowment in 2004 and were thus the trusters selected for our analysis. This selection is based on their level of trust. In order to see if and how our experimental samples differ we compare their socio-economic characteristics with those of sample F of the SOEP (Table 4.1), which is a random sample of the German population. Both sub-samples, the first-mover and the second-mover sample, closely follow the distribution of the German population as a whole with a few minor exceptions.

## 4.3 The Effect of Experienced Trustworthiness on Trusting Behaviour

We examine the extent to which negative or positive experiences affect trusting behaviour. To test this we regress the change of trust between 2004 and 2005 on to the positive or negative experience in 2004 using OLS[6]. We find that if trusters are betrayed in 2004, which is exogenous to their decision in 2004, trusters' trust decreases in 2005 compared to 2004 (Regression 1 in Table 4.2). A received back-transfer of zero in 2004 reduced trust by almost 1 point on average, that is, 20% of the previous year's trust. Furthermore, we control for socio-economic changes such as changes in income, the employment status, whether a person moved home, and bereavement (partner, mother or father). We find that the coefficient of negative experience hardly changes and is still significant at the 5% level when controlling for these changes (Regression 2 in Table 4.2). Concerning the socio-economic changes we find that even a 100% increase in the household income between the years increases trust only by 0.52 points. Similarly, moving home and changes in employment or retirement status also do not have particularly strong effects, suggesting that last year's experience has a very fundamental influence on behaviour that is more influential than important changes in life conditions. Further controlling for socio-economic characteristics does not affect the influence of the negative experience on trust (Regression 3 in Table 4.2). The coefficient of negative experience is slightly smaller but remains significant at the 5% level.

A positive experience, however, does not increase trust. The coefficient is almost zero and far from being significant. Thus we find that a lack of trustworthiness (negative experience) decreases people's trust, and very kind behaviour (positive experience) has no effect on people's trust.

Next, we analyse if the influence of the negative experience depends on its magnitude,

[6]Taking into consideration that trusters could maximally increase their trust by 5 points we run a tobit regression. The results are essentially the same as with OLS.

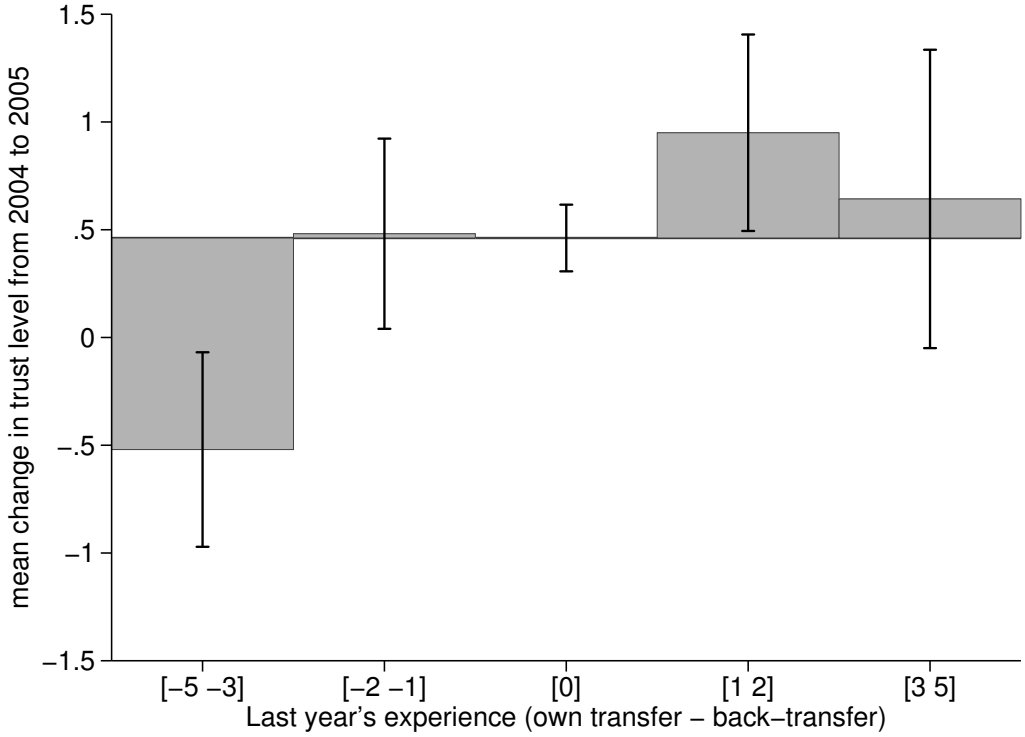
Table 4.2: Experience affects trusting behaviour

| Dep. var.: change in trust | (1) | (2) | (3) |
|---|---|---|---|
| Negative experience [1 5] (Base: neutral experience) | −0.24⋆ | −0.23⋆ | −0.21⋆ |
| | (0.10) | (0.10) | (0.10) |
| Positive experience [1 5] | 0.03 | 0.02 | 0.05 |
| | (0.12) | (0.12) | (0.12) |
| Change in log HH income adj. for HH-size | | 0.49⋆ | 0.51⋆ |
| | | (0.21) | (0.22) |
| Is newly employed | | −0.97 | −1.16 |
| | | (0.73) | (0.76) |
| Is newly unemployed | | −0.54 | 0.92 |
| | | (0.85) | (1.08) |
| Is newly non-labour market/retired | | 0.54 | 0.56 |
| | | (0.69) | (0.74) |
| Has moved home | | −1.29° | −1.31° |
| | | (0.74) | (0.74) |
| Bereavement (partner, mother or father) | | 0.42 | 0.21 |
| | | (0.92) | (0.97) |
| Log household income adjusted for HH-size | | | 0.55° |
| | | | (0.33) |
| Employment status: unemployed (Base: employed) | | | −1.23 |
| | | | (0.76) |
| Employment status: self-employed | | | 0.52 |
| | | | (0.69) |
| Employment status: non labour market/retired | | | 0.29 |
| | | | (0.43) |
| Dummy of being female | | | −0.44 |
| | | | (0.28) |
| Age | | | 0.02 |
| | | | (0.06) |
| $Age^2$ | | | −0.00 |
| | | | (0.00) |
| High school (Base: less than high school) | | | 0.42 |
| | | | (0.43) |
| More than high school | | | 0.82 |
| | | | (0.53) |
| Resident in East Germany in 1989 (Base: West Germany) | | | −0.31 |
| | | | (0.35) |
| Resident abroad in 1989 | | | 0.05 |
| | | | (0.65) |
| Foreign nationality | | | 0.94 |
| | | | (0.72) |
| Marital status: single (Base: married) | | | 0.44 |
| | | | (0.53) |
| Marital status: divorced | | | 0.16 |
| | | | (0.51) |
| Marital status: widowed | | | 0.44 |
| | | | (0.61) |
| Constant | 0.54⋆⋆ | 0.56⋆⋆ | −5.43 |
| | (0.15) | (0.16) | (3.59) |
| Observations | 242 | 242 | 242 |
| Adjusted-$R^2$ | 0.02 | 0.04 | 0.08 |

Notes. OLS regression, coefficients and standard errors (in parentheses) reported. °, ⋆, ⋆⋆ denote statistical significance on the 10%, 5% and 1% level, respectively. Negative and positive experience is defined as the difference between the level of trust and the received back-transfer in 2004.

i.e. if the effect of a small negative experience differs from the effect of large negative experience. In order to analyse this, we create two new dummy variables. One takes the value of 1 if the difference between last year's transfer and the received transfer is either 1 or 2 and the other dummy variable takes the value 1 if the difference is 3,4, or 5 points. We chose these groups because with a difference of 1 or 2 points the truster earns more than his initial endowment of 10 Euros, whereas for a larger difference his/her earnings are lower than 10 Euros. Thus, the latter is a measure for a strong negative experience

Figure 4.1: The effect of experienced trustworthiness on trust




Notes. The bars represent the mean change in trust from 2004 to 2005, aligned by the experience in 2004. The bars illustrate the mean difference in the change of trust compared to the baseline group, who received the same back-transfer as what they had sent. The error bars denote +/- the standard error of the mean.

and the former for a weak one. This equates with absolute and relative untrustworthiness as introduced in section 4.1. We find that people who are exposed to a weak negative experience do not change their level of trust compared to a 'neutral' experience (Figure 4.1 and regression 1 in Table 4.3). However, those who were exposed to a strong negative experience reduce their trust by 0.5 points. Compared to an increase of 0.5 points for people with a 'neutral' experience this is a difference of almost 1 point. Note that the size of the effect of the highest possible negative experience (-5 points) is about the same with both the linear and non-linear model.

Table 4.3: Strong and weak experience

| Dep. var.: change in trust | (1) |
|---|---|
| Negative experience: strong [3 5] (Base: neutral) | $-0.98^{\star}$ |
| | (0.44) |
| Negative experience: weak [1 2] | 0.02 |
| | (0.43) |
| Positive experience: strong [3 5] | 0.18 |
| | (0.57) |
| Positive experience: weak [1 2] | 0.49 |
| | (0.49) |
| Constant | $0.46^{\star\star}$ |
| | (0.16) |
| Observations | 242 |
| Adjusted-$R^2$ | 0.01 |

Notes. OLS regression, coefficients and standard errors (in parentheses) reported. $^{\circ}$, $^{\star}$, $^{\star\star}$ denote statistical significance on the 10%, 5% and 1% level, respectively.

In sum, we find that experienced untrustworthiness results in a strong and significant negative decrease in trusting behaviour. This effect does not change when including socio-economic changes and characteristics.

### 4.3.1 What drives the change in trust?

The question arises why people change their trust based on one interaction that happened a year ago? Remember that participants are explicitly told on the decision sheet that the trustee/truster will be a different person than last year. This rules out possible reputation effects. However, playing a trust game is something new for the vast majority of people in this world. Thus, trusters do not really know how others behave in the role of trustees in a trust game. People may have only a vague idea on how many people are trustworthy. Thus every experience is valuable information and people should update their prior beliefs with this new information.

In order to test if trusters updated their beliefs based on their experience in the trust game, we also measured trusters' beliefs about the behaviour of trustees. We confronted them with three hypothetical situations. What would they expect the trustee to transfer back if they transferred 0, 5, or 10 points? Since we measured expectations in both years[7], we can analyse if the expected back-transfer is affected by the subjects'

[7]The expected back-transfer at a first-mover transfer of 5 points was only asked in 2005. For the analysis in Table 4.4, we imputed the expectation for 2004 based on the expectation at 0 and 10 points assuming a linear relationship.

Table 4.4: Experience affects beliefs

| Dependent variable: | at a first-mover transfer of ... | | | | | |
|---|---|---|---|---|---|---|
| change in expected back-transfer | 0 points | | 5 points | | 10 points | |
| Negative experience [1 5] | −0.38⋆ | −0.36⋆ | −0.33⋆⋆ | −0.31⋆⋆ | −0.33⋆ | −0.30⋆ |
| (Base: neutral experience) | (0.18) | (0.18) | (0.10) | (0.10) | (0.13) | (0.13) |
| Positive experience [1 5] | 0.26 | 0.35 | −0.14 | −0.10 | −0.12 | −0.15 |
| | (0.22) | (0.22) | (0.13) | (0.13) | (0.15) | (0.16) |
| Change in log household income | | 0.56 | | −0.61 | | −0.95 |
| (adjusted for household size) | | (0.39) | | (0.58) | | (0.72) |
| Is newly employed | | −1.56 | | −1.47° | | −0.31 |
| | | (1.30) | | (0.78) | | (0.98) |
| Is newly unemployed | | 0.89 | | −0.60 | | 0.85 |
| | | (2.19) | | (1.33) | | (1.46) |
| Is newly non-labour market/retired | | −0.97 | | −0.97 | | −0.01 |
| | | (1.26) | | (0.75) | | (0.95) |
| Has moved home | | −0.46 | | −0.70 | | −0.17 |
| | | (1.35) | | (0.80) | | (0.96) |
| Bereavement (partner, mother, father) | | −0.74 | | 0.34 | | 1.97 |
| | | (1.67) | | (1.00) | | (1.25) |
| Log HH income adjusted for HH-size | | −0.08 | | −0.02 | | 0.20 |
| | | (0.58) | | (0.38) | | (0.48) |
| Employment status: unemployed | | 0.62 | | 1.08 | | −0.85 |
| (Base: employed) | | (1.35) | | (0.87) | | (0.98) |
| Employment status: self-employed | | 1.51 | | 0.85 | | 0.27 |
| | | (1.26) | | (0.75) | | (0.95) |
| Employment status: non labour market | | −0.00 | | 0.08 | | 0.36 |
| and retired | | (0.74) | | (0.44) | | (0.55) |
| Dummy of being female | | −1.58⋆⋆ | | −0.21 | | 0.43 |
| | | (0.49) | | (0.29) | | (0.37) |
| Age | | 0.00 | | −0.11 | | −0.02 |
| | | (0.11) | | (0.07) | | (0.08) |
| $Age^2$ | | −0.00 | | 0.00 | | −0.00 |
| | | (0.00) | | (0.00) | | (0.00) |
| High school | | −1.61⋆ | | −0.37 | | 0.32 |
| (Base: less than high school) | | (0.75) | | (0.45) | | (0.57) |
| More than high school | | −0.60 | | 0.37 | | 0.69 |
| | | (0.93) | | (0.55) | | (0.70) |
| Resident in East Germany in 1989 | | 0.47 | | −0.09 | | 0.31 |
| (Base: West Germany) | | (0.63) | | (0.38) | | (0.47) |
| Resident abroad in 1989 | | 0.78 | | −0.19 | | 0.29 |
| | | (1.15) | | (0.69) | | (0.83) |
| Foreign nationality | | −0.69 | | 0.10 | | −0.63 |
| | | (1.27) | | (0.76) | | (0.93) |
| Marital status: single | | 1.22 | | 0.16 | | 0.21 |
| (Base: married) | | (0.91) | | (0.54) | | (0.68) |
| Marital status: divorced | | 0.16 | | −0.02 | | 0.25 |
| | | (0.95) | | (0.58) | | (0.70) |
| Marital status: widowed | | −0.53 | | 0.21 | | 0.22 |
| | | (1.09) | | (0.65) | | (0.82) |
| Constant | 0.39 | 3.35 | 0.47⋆⋆ | 4.43 | −0.50⋆ | −1.92 |
| | (0.27) | (6.27) | (0.16) | (3.96) | (0.19) | (5.02) |
| Observations | 231 | 231 | 231 | 231 | 232 | 232 |
| Adjusted-$R^2$ | 0.02 | 0.08 | 0.04 | 0.08 | 0.02 | −0.00 |

Notes. OLS regression, coefficients and standard errors (in parentheses) reported.

°, ⋆, ⋆⋆ denote statistical significance on the 10%, 5% and 1% level, respectively.

experiences.

We run an OLS regression in which we regressed the change of the expected back-transfers on to the experience and other control variables. We find that a negative experience in 2004 decreases the amount of points trusters expect back from the trustees in 2005 for all three transfer levels (Table 4.4). The expected back-transfer for a first-mover transfer of 10 and 5 points reduces by 1.5 points after a strong negative experience, the expectation at 0 points by 1.8 points. A positive experience does not influence the expectations in any significant way. Concerning socio-economic changes we do not find any strong effects. We find a marginally significant negative effect for newly employed people.

In sum, we showed that the experience in the trust game in 2004 affected trusting behaviour and we showed that it affects expectations about the trustworthiness of the second-mover. It is now interesting to see if the influence of the experience on behaviour is fully explained by changed expectations. We therefore regress the change in trusting behaviour on the experience in 2004 and the change of expectations.

For the expectations we created two measures which are easier to interpret than the raw expected back-transfer. For every person, we regressed the three expected back-transfers on the respective first-mover transfers. If a person expected the same amount back than what he/she sent, the slope of the fitted line would be one and the constant would be zero. The slope and the constant have a straightforward interpretation. The latter can be thought of as measuring expected generosity and the slope the expected reciprocity. Thus, one would predict that the higher the expected generosity and reciprocity is, the higher the truster's transfer. As expected, the changes in expectations have a significant influence on the change in trusting behaviour (Regression 2 in Table 4.5). A 1 point increase in expected generosity significantly increases trusting behaviour by 0.19 points and an increase in the expected reciprocity from 0 to 1 increases trusting behaviour by 1.72 points.

To see if experience directly influences trusting behaviour or only expectations, we regress the change in trusting behaviour on 2004's experience and the change in expectations. We find that the coefficient of negative experience decreases from 0.26 to 0.20 when controlling for the change in expectations (Regression 1 and 3 in Table 4.5). However, the influence of negative experience is still significant at the 5%-level. Controlling for socio-economic changes slightly reduces the coefficient of negative experience to 0.19 and is now only significant at the 10%-level (Regression 4 in Table 4.5). Further controlling for socio-economic characteristics does not change the results in an important way (Regression 5 in Table 4.5). To compare the magnitude of the experience and the expectations effect, we also perform the same analysis with standardised coefficients.

Table 4.5: Change in trusting behaviour

| Dependent variable: change in trusting behaviour | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Negative experience [1 5] (Base: neutral experience) | −0.26⋆ | | −0.20⋆ | −0.19° | −0.17° |
| | (0.10) | | (0.10) | (0.10) | (0.10) |
| Positive experience [1 5] | 0.11 | | 0.08 | 0.09 | 0.08 |
| | (0.12) | | (0.12) | (0.12) | (0.12) |
| Change in expected generosity [-10 10] | | 0.19⋆⋆ | 0.16⋆ | 0.16⋆ | 0.16⋆ |
| | | (0.07) | (0.07) | (0.07) | (0.07) |
| Change in expected reciprocity [-2 2] | | 1.72⋆⋆ | 1.56⋆⋆ | 1.58⋆⋆ | 1.69⋆⋆ |
| | | (0.38) | (0.39) | (0.39) | (0.41) |
| Change in log HH income adj. for HH-size | | | | 0.11 | −0.63 |
| | | | | (0.48) | (0.54) |
| Is newly employed | | | | −0.85 | −0.90 |
| | | | | (0.70) | (0.72) |
| Is newly unemployed | | | | 0.22 | 1.12 |
| | | | | (0.98) | (1.21) |
| Is newly non-labour market/retired | | | | 0.78 | 0.81 |
| | | | | (0.66) | (0.70) |
| Has moved home | | | | −0.83 | −0.71 |
| | | | | (0.74) | (0.75) |
| Bereavement (partner, mother or father) | | | | 0.62 | 0.15 |
| | | | | (0.88) | (0.93) |
| Log household income adjusted for HH-size | | | | | 0.97⋆⋆ |
| | | | | | (0.36) |
| Employment status: unemployed (Base: employed) | | | | | −0.70 |
| | | | | | (0.76) |
| Employment status: self-employed | | | | | −0.49 |
| | | | | | (0.70) |
| Employment status: non labour market/retired | | | | | 0.16 |
| | | | | | (0.41) |
| Dummy of being female | | | | | −0.19 |
| | | | | | (0.28) |
| Age | | | | | −0.01 |
| | | | | | (0.06) |
| $Age^2$ | | | | | 0.00 |
| | | | | | (0.00) |
| High school (Base: less than high school) | | | | | 0.57 |
| | | | | | (0.42) |
| More than high school | | | | | 0.81 |
| | | | | | (0.52) |
| Resident in East Germany in 1989 (Base: West Germany) | | | | | −0.23 |
| | | | | | (0.35) |
| Resident abroad in 1989 | | | | | 0.09 |
| | | | | | (0.64) |
| Foreign nationality | | | | | 1.37° |
| | | | | | (0.70) |
| Marital status: single (Base: married) | | | | | −0.03 |
| | | | | | (0.50) |
| Marital status: divorced | | | | | 0.33 |
| | | | | | (0.52) |
| Marital status: widowed | | | | | 0.72 |
| | | | | | (0.61) |
| Constant | 0.56⋆⋆ | 0.38⋆⋆ | 0.47⋆⋆ | 0.46⋆⋆ | −9.12⋆ |
| | (0.15) | (0.13) | (0.15) | (0.16) | (3.70) |
| Observations | 230 | 230 | 230 | 230 | 230 |
| Pseudo-$R^2$ | 0.03 | 0.07 | 0.09 | 0.08 | 0.13 |

Notes. OLS regression, coefficients and standard errors (in parentheses) reported.
°, ⋆, ⋆⋆ denote statistical significance on the 10%, 5% and 1% level, respectively.

The results clearly reveal that the change in expectations is the most important channel for a change in trust behaviour: For example, an increase in expected reciprocity by one standard deviation increases trust by 0.79 points, but an increase in negative experiences by one standard deviation decreases trust only by 0.25 points.

Thus, negative experiences have an important effect on trusting behaviour through their impact on belief formation, i.e. on expectations; but beyond this, they have a further independent effect on trusting behaviour. Based on economic theory one would suggest that apart from expectations, preferences determine behaviour. Among the more important preferences determining trusting behaviour are risk and social preferences (see chapter 3). We explore if these preferences moderate the remaining effect of experience on trusting behaviour. Besides discussing this possibility in the next section we will also investigate for what types of people the belief formation channel is important.

### 4.3.2 Attitudes, types and preferences

In the previous section we showed that negative experience induces a change in behaviour. A question arising at this point is if everyone was affected in the same way by such experiences or whether we can identify systematic differences. The answer to this question is important to gain a deeper understanding of the mechanisms that contribute to a self-reinforcing or self-destroying propagation of trust or mistrust within a society.

What types of people are most likely to change their expectations? One would expect that people who have low self-assurance are more likely to change their expectation based on new information. We use two proxies for self-assurance. On the one hand, we measure the degree of worry about an individual's financial situation and his/her health condition and calculated the mean answer to these two questions. This gives us an indication of how much a person worries about his/her own situation. On the other hand we use the agreement to the statement '*How my life goes depends on me*' as a proxy for self-assurance. The higher the agreement to this statement the less likely we expect a person to change their expectations.

We find that people who show low self-assurance (lots of worries and disagreement with the statement) change their expectations much more than those with high self-assurance (Table 4.6). In detail, an increase by one standard deviation in people's worries increases the effect of a strong negative experience from -2.72 to -4.47 points for the expectation at 10 points. Conversely, an increase by one standard deviation in the agreement to the statement '*How my life goes depends on me*' reduces the effect of a strong negative experience from -2.72 to -0.05 points (Regression 3 in Table 4.6). The changes in expectations at 0 and 5 points do not significantly depend on self-assurance.

Table 4.6: Self-assurance mediates the change in expectations

| Dep. var.: change in expected back-transfer | at a first-mover transfer of ... | | |
|---|---|---|---|
| | 0 points | 5 points | 10 points |
| Negative experience: strong [3 5] (Base: neutral) | −1.26⋆ | −1.42⋆⋆ | −2.72⋆⋆ |
| | (0.61) | (0.50) | (0.83) |
| Worried * strong negative experience | −0.30 | −0.27 | −1.75⋆ |
| | (0.59) | (0.48) | (0.80) |
| Life control * strong negative experience | −0.58 | −0.19 | 2.67⋆ |
| | (0.80) | (0.66) | (1.08) |
| Negative experience: weak [1 2] | 0.11 | 0.12 | −0.27 |
| | (0.55) | (0.46) | (0.74) |
| Worried * weak negative experience | −0.98 | −0.38 | 0.57 |
| | (0.60) | (0.49) | (0.81) |
| Life control * weak negative experience | 1.39⋆ | 0.17 | −0.64 |
| | (0.59) | (0.49) | (0.80) |
| Positive experience: strong [3 5] | −0.40 | −0.52 | 1.50 |
| | (0.76) | (0.66) | (1.03) |
| Positive experience: weak [1 2] | −0.24 | −0.21 | 0.47 |
| | (0.61) | (0.50) | (0.82) |
| Worried about own finances and health (std.) | 0.01 | 0.14 | 0.31 |
| | (0.19) | (0.15) | (0.25) |
| How my life goes depends on me (std.) | −0.07 | 0.01 | −0.03 |
| | (0.18) | (0.16) | (0.26) |
| Constant | −0.54⋆ | 0.42⋆ | 0.36 |
| | (0.21) | (0.17) | (0.28) |
| Observations | 231 | 230 | 230 |
| Adjusted-$R^2$ | 0.02 | 0.01 | 0.06 |

Notes. OLS regression, coefficients and standard errors (in parentheses) reported. °, ⋆, ⋆⋆ denote statistical significance on the 10%, 5% and 1% level, respectively.

As we have seen above, the change in expectations cannot fully capture the effect of experience on trusting behaviour. That is, negative experience affects behaviour in some way which is independent of expectations. In order to gain further insights into this remaining unexplained influence of experience on trusting behaviour, we consider if different types of preferences moderate this remaining effect. Specifically we explore risk and social preferences which have been shown to influence trusting behaviour (chapter 3). We use a survey question shown to be a valid measure (Dohmen *et al.*, forthcoming; Roe *et al.*, 2009) for capturing risk preferences. Therein people rate their risk aversion on a scale from 0 to 10. Regarding social preferences we measure two aspects. On the one hand we measure betrayal aversion, which is shown to be related to trusting behaviour (Bohnet and Zeckhauser, 2004). As a proxy for betrayal aversion we measure how strongly people obey a norm of negative reciprocity. This survey measure was

developed by Perugini *et al.* (2003). On the other hand we consider the desire for an appreciation of one's kind actions towards others as a measure for social preferences. Since appreciation of one's engagement is often the only reward/incentive for voluntary work, volunteering promises to be a good proxy for measuring this concept. We measure how frequently people volunteer in clubs and social services. In order to differentiate the desire for an appreciation from pure socialising we include the frequency of socialising with friends and relatives.

We now regress the change in trusting behaviour on the experience and its interaction with risk and social preferences while controlling for the change in expectations. We find that the degree of risk aversion does not influence the impact of experience on trusting behaviour. However, after a strong negative experience, betrayal-averse people exhibit a 2.31 points lower trust level compared to the baseline group, i.e. people with low social preferences who had a strong negative experience. The interaction effect 'negative experience x volunteering' is even stronger. Volunteers' level of trust is 3.4 points lower than the baseline group. Thus, since the effect of experiences on trust differs substantially depending on people's social preferences, it appears that social preferences are strong moderators of the effect of experience on trust.

The question arises how to interpret this result. Why do people with social preferences react differently to experience than others? A possible answer is offered by the 'Discovered Preference Hypothesis' (DPH) first introduced by Plott (1996). According to this hypothesis people are not fully aware of their preferences, or they do not know that a certain preference is relevant to a particular situation. Through experience people become aware of what their preferences really are or they discover that a certain preference also matters in a new situation (Braga and Starmer, 2005; Loomes *et al.*, 2003; Plott, 1996; Smith, 1994). Plott (1996) noted:

> *The hypothesis suggests that attitudes like expectations, beliefs, risk-aversion and the like, are discovered, as are other elements of the environment. People acquire an understanding of what they want through a process of reflection and practice.* (p. 227)

In other words, people who are confronted with a decision that is new to them do not yet know their own preferences for that particular situation very well. Experiencing the consequences of one's own actions in such a situation shapes an individual's preferences or enables people to discover that they have a certain preference. Clearly, the DPH offers possible interpretations for our results. Participation in a trust game is an unusual and new experience for most people. Therefore, people might initially have only a vague sense of how they might feel in case their trust is rewarded or abused. They might then go on to discover in their first experiences of the set-up that they do not like to be taken

Table 4.7: Preferences mediate influence of experience

| Dep. var.: change in trust | (1) | (2) |
|---|---|---|
| Negative experience: strong [3 5] (Base: neutral) | −0.78° | 0.59 |
| | (0.44) | (0.76) |
| Risk aversion * strong negative experience | | 0.54 |
| | | (0.46) |
| Betrayal aversion * strong negative experience | | −1.72⋆ |
| | | (0.85) |
| Volunteering * strong negative experience | | −2.81⋆⋆ |
| | | (0.99) |
| Socialising * strong negative experience | | 0.58 |
| | | (0.86) |
| Negative experience: weak [1 2] | −0.22 | −1.71⋆ |
| | (0.42) | (0.81) |
| Risk aversion * weak negative experience | | −0.47 |
| | | (0.49) |
| Betrayal aversion * weak negative experience | | 1.29 |
| | | (0.91) |
| Volunteering * weak negative experience | | 0.82 |
| | | (0.86) |
| Socialising * weak negative experience | | 1.55° |
| | | (0.88) |
| Positive experience: strong [3 5] | 0.19 | 0.19 |
| | (0.61) | (0.60) |
| Positive experience: weak [1 2] | 0.35 | 0.43 |
| | (0.46) | (0.46) |
| Change in expected generosity [-10 10] | 0.16⋆ | 0.17⋆ |
| | (0.07) | (0.07) |
| Change in expected reciprocity [-2 2] | 1.52⋆⋆ | 1.45⋆⋆ |
| | (0.37) | (0.38) |
| Risk aversion (std.) | | −0.04 |
| | | (0.15) |
| Betrayal aversion (Dummy) | | 0.09 |
| | | (0.30) |
| Dummy of volunteering at least sometimes | | 0.49° |
| | | (0.29) |
| Dummy of socialising at least weekly | | −0.11 |
| | | (0.29) |
| Constant | 0.48⋆⋆ | 0.29 |
| | (0.16) | (0.26) |
| Observations | 228 | 228 |
| Adjusted-$R^2$ | 0.08 | 0.11 |

Notes. OLS regression, coefficients and standard errors (in parentheses) reported.
°, ⋆, ⋆⋆ denote statistical significance on the 10%, 5% and 1% level, respectively.

advantage of in this particular situation. In other words, they might find out that they do indeed care. In this light, our results can be interpreted insightfully: Experienced untrustworthiness makes people aware that they do not like to be taken advantage of, nor do they take lightly the non-appreciation of their trust, even in this particular setting.

Returning to the original question, we showed that after a strong negative experience, people who lack self-assurance are more likely to adjust their expectations about the behaviour of the trustee. Furthermore, we showed that, controlled for a change in expectations, betrayal-aversion as well as the desire for appreciation for kind actions strongly increase the impact of a strong negative experience.

## 4.4 The Effect of Trust on Trustworthy Behaviour

The question whether an increase of trust is rewarded with a higher probability of trustworthy behaviour has provoked several studies, as discussed in section 4.1. However, as we have discussed, the approaches taken cannot convincingly show a causal relationship, the main reason being that the design of the trust game is not suitable for a cross-section analysis of the question of interest. In order to solve this problem we took the following approach.

We exogenously increased the trust placed in randomly selected panel participants from one year to the next. We then analysed if the probability of trustworthiness is higher for those people in whom more trust was placed in 2005 than in 2004, compared to people who received the same level of trust in 2004 and 2005. Thus we ask the following question: Does the experience of a higher level of trust than earlier increase the probability of being trustworthy compared to the experience of a constant level of trust?

To ensure that the change in trust is not endogenous to the observation of trustworthiness, we select a group which is characterised by all having received the same level of trust in 2005, namely 5 points, from the whole population of second-movers. Thus, the only difference between the second-movers in our sample is that they experienced a different level of trust a year earlier in 2004. It was common knowledge that the trust was placed by two different first-movers. Since the time-span of one year between the two placements of trust is quite long, we again exclude short-term emotional responses and reduce a potential experimenter demand effect. If we still find an effect this will then be a strong indicator that there is a relation between a change in trust and trustworthy behaviour.

We regress a dummy variable that indicates if someone behaved trustworthily[8] or not

[8]As outlined in section 4.2 someone is defined to be trustworthy if her/his back-transfer is at least

Table 4.8: Influence of Experienced Change in Trust on Trustworthiness

| Dep. variable: Dummy for Behaving Trustworthy | (1) |
|---|---|
| Increase in trust: strong [3 5] (Base: no change in trust) | 0.15$^{\star}$ |
| | (0.05) |
| Increase in trust: weak [1 2] | −0.12 |
| | (0.09) |
| Decrease in trust: strong [-3 -5] | 0.09 |
| | (0.06) |
| Decrease in trust: weak [-1 -2] | 0.06 |
| | (0.09) |
| Change in log HH income adj. for HH-size | 0.03 |
| | (0.10) |
| Has moved home | −0.04 |
| | (0.11) |
| Log household income adjusted for HH-size | 0.02 |
| | (0.06) |
| Employment status: unemployed (Base: employed) | −0.15 |
| | (0.13) |
| Employment status: non labour market/retired | −0.02 |
| | (0.08) |
| Dummy of being female | 0.01 |
| | (0.06) |
| Age | −0.00 |
| | (0.01) |
| Age$^2$ | 0.00 |
| | (0.00) |
| High school (Base: less than high school) | 0.09 |
| | (0.08) |
| More than high school | 0.15$^{\circ}$ |
| | (0.06) |
| Resident in East Germany in 1989 (Base: West Germany) | −0.03 |
| | (0.08) |
| Resident abroad in 1989 | −0.01 |
| | (0.13) |
| Foreign nationality | −0.01 |
| | (0.12) |
| Marital status: single (Base: married) | −0.09 |
| | (0.10) |
| Marital status: divorced | −0.11 |
| | (0.11) |
| Marital status: widowed | −0.09 |
| | (0.14) |
| N | 234 |
| Pseudo-R$^2$ | 0.11 |
| Prob(dep.var = 1) | 0.80 |

Notes. Probit regression, marginal effects and standard errors (in parentheses) reported. $^{\circ}$, $^{\star}$, $^{\star\star}$ denote statistical significance on the 10%, 5% and 1% level, respectively.

in 2005 on the change of trust placed in these trustees from 2004 to 2005 and on socio-economic controls[9]. Based on a probit regression, we find that 80% of second-movers behave trustworthily if the trust placed in them did not change over the time interval. If the trust strongly increased from 2004 to 2005 (3 to 5 points) the probability that a second-mover is trustworthy also increases significantly from 80% to 95% (Regression 1 in Table 4.8). A small increase (1 to 2 points) decreases the probability to 68%, but this effect is not significant. A decrease in trust over one year hardly changed trustworthy behaviour. Socio-economic characteristics have almost no explanatory power for trustworthy behaviour. We find a weak positive effect of education and a weak effect of religion in that Catholics are more likely to be trustworthy than Protestants.

In sum we find that a strong increase in trust is rewarded with an increased trustworthiness compared with no change in trust. A small increase in trust is not rewarded. Interestingly, we find again that the effect of a positive experience is stronger than the effect of the socio-economic characteristics included.

On the question why people respond with higher trustworthiness when trust increases, we can only speculate. One possible mechanism is that people reciprocate with a higher trustworthiness to an increase in trust placed in them. Note that in 2005 the trust experienced by all the second-movers in our sample is the same. The only difference is the trust placed in them in 2004. Since we made it very clear that the first-mover is a different person than the year before[10], the first-mover's action in 2005 is evaluated based on the experienced first-mover decision a year earlier. This is not surprising against the background that playing a trust game is a new situation for respondents and a second-mover cannot evaluate first-movers' behaviour against a long experience with other first-movers. Based on this reasoning one can conclude that an increase in trust increases trustworthiness in those circumstances where trustees perceive the level of trust they receive as higher than what they 'normally' receive.

## 4.5 Conclusions

Because of the importance of trust for many economic and social relationships it is important to understand how levels of trust change over time and to understand the mechanisms that contribute to a self-reinforcing or self-destroying propagation of trust or mistrust within a society. Theoretically, experience of untrustworthiness is claimed to be an important force that drives changes in trust. However, to the best of our

as big as the transfer received from the truster.

[9]Some socio-economic variables were not included due to too few observations per category.

[10]It was written in bold in the instructions and a second time on the decision sheet

knowledge, there is no empirical, causal evidence on whether experience can actually change trusting *behaviour*. In this chapter we address this gap by exploring whether an exogenous experience of untrustworthiness changes trusting behaviour.

We demonstrated that experienced untrustworthiness causes a significant decrease in trusting behaviour. This effect is independent of important socio-economic changes such as changes in income and employment status. We find that the main channel through which experience affects trusting behaviour is by beliefs. An experience of untrustworthiness decreases the belief about others' trustworthiness which in turn decreases trusting behaviour. The change in beliefs is strongest for people of low self-assurance. However, the effect of experienced untrustworthiness on behaviour cannot be fully explained by changing beliefs. It is unclear what precisely the remaining effect is. However, we do find that it is strongly moderated by social preferences. It is strongest for people with pronounced social preferences and inexistent for people with weak social preferences. A possible interpretation we offer is in relation to the 'Discovered Preference Hypothesis': Through experience people discover that their social preferences are indeed salient in the situation of the experimental trust game, leading them to adapt their behaviour in the later game. An indication of this result's importance can perhaps be found in the fact that the magnitude of the effect of experience on trusting behaviour by far surpasses the effects of any other factors we found to be influential. For instance, we find that an increase in people's household income is related to an increase in trusting behaviour but its magnitude is much lower than that of the effect of experienced untrustworthiness. A 10% increase in income only increases trust by 1%, whereas experience of untrustworthiness decreases trust by more than 20%.

Similarly, an increase in experienced trust increases the likelihood of observing trustworthy behaviour. In contrast, socio-economic changes and characteristics are not related to trustworthy behaviour.

Both experiences, of trust and of untrustworthiness, have a strong impact on behaviour. This is especially remarkable since there is a large time gap of one year between the experience and our second behavioural measure. This is further evidence that the experience of untrustworthiness is a powerful determinant of trusting behaviour, and it is actually more important than other relevant changes in life. Thus, social interactions have their own dynamics and give rise to self-reinforcing or self-destroying propagation of trust or mistrust within a society.

Taken together, our results suggest that the relation of trust and trustworthiness might be in the form of a vicious circle, where less trust leads to less trustworthiness, which again leads to less trust. Although other factors do also influence trusting behaviour, the influence of untrustworthiness is the strongest influence we found. Thus

even small scale, individual experiences of untrustworthiness can possibly, by means of the vicious circle, be an important aspect for understanding decreases in aggregate levels of trust. On the other hand, the fact that we found no evidence of a positive effect of experienced trustworthiness on trusting behaviour, and only minor effects by other factors, raises the question of how to explain observed increases in trust. Clearly, more research is needed, particularly to understand not just decreases, but also increases in trust.

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# 4.A Participants' Instructions for 2005

TNS Infratest Sozialforschung Deutsches Institut für Wirtschaftsforschung

Leben in Deutschland

## Verhaltensexperiment „Geben und Nehmen"

Zwei Personen,
die sich nicht kennen,
treffen jeder eine Entscheidung
über die Verwendung von Geld
und erzielen zusammen ein Ergebnis.

### Spielregeln: So wird's gemacht!

Es gibt zwei Teilnehmer.
Jeder Teilnehmer erhält einen Anfangsbetrag von 10 Punkten.

Beide Teilnehmer können ihre 10 Punkte
- zwischen sich und der anderen Person aufteilen oder
- ganz für sich behalten oder
- ganz an die andere Person weitergeben.

Für jeden Punkt, den Sie behalten, bekommen Sie selbst 1 €. 

Für jeden Punkt, den Sie weitergeben, bekommt die andere Person 2 €. 

Umgekehrt gilt:
Für jeden Punkt, den die andere Person an Sie weitergibt, bekommen Sie 2 €

**Dies bedeutet für beide Teilnehmer:**
**Behalten: 0 Punkte = 0€ / 1 Punkt = 1€ / 2 Punkte = 2€ / 3 Punkte = 3€ *usw. bis* / 10 Punkte = 10€**
**Weitergeben: 0 Punkte = 0€ / 1 Punkt = 2€ / 2 Punkte = 4€ / 3 Punkte = 6€ *usw. bis* / 10 Punkte = 20€**

**Sie sind Teilnehmer 1, entscheiden also als Erster.**

Das heißt, Sie entscheiden, wie viele von Ihren 10 Punkten Sie an die andere Person (Teilnehmer 2) weitergeben und wie viele Sie behalten.

*Teilnehmer 2 ist eine **andere Person** als letztes Jahr!*

Teilnehmer 2 wird nie erfahren wer Sie sind, aber wir werden Ihre Entscheidung an diese Person übermitteln.

Teilnehmer 2 weiß also, wie viele Punkte Sie ihm gegeben haben, wenn er nun seine Entscheidung über die Aufteilung der 10 Punkte trifft.

Aus beiden Entscheidungen zusammen errechnet sich der Geldbetrag, den Sie erzielen. Diesen Geldbetrag werden wir Ihnen per Verrechnungsscheck zusenden. Ihre Teilnahme ist freiwillig. Der Rechtsweg ist ausgeschlossen.

**Um Ihre Punkte einzutragen, drehen Sie das Blatt bitte um.** ➡

- 1 -

## Entscheidungsblatt für Teilnehmer 1

Wie teilen Sie die **10 Punkte** auf?

Das heißt, wie viele Punkte **(von 0 bis 10)** geben Sie an Teilnehmer 2 weiter und wie viele behalten Sie?

*Ich gebe* ☐ *Punkte weiter*

*und behalte* ☐ *Punkte*

zusammen also 10 Punkte.

***Teilnehmer 2 ist eine andere Person als letztes Jahr!***

**Und so geht's weiter:**

Infratest übermittelt nun die Punktezahl, die von Ihnen weitergegeben wurde, an Teilnehmer 2.

Teilnehmer 2 entscheidet daraufhin, wie viele Punkte er an Sie gibt.

„Was wäre wenn …" Die bekannte Frage aus dem Alltag bewegt auch die Wissenschaft. Auch im Wirtschaftsleben ist es häufig notwendig, vorherzusagen, wie sich andere Menschen verhalten. Daher bitten wir Sie, hier noch eine Zusatzfrage dazu zu beantworten:

Was denken Sie, wie würde sich Teilnehmer 2 in den drei folgenden Beispielen verhalten?

Wenn ich ihm 10 Punkte gebe, erhalte ich von ihm ☐ Punkte

Wenn ich ihm 5 Punkte gebe, erhalte ich von ihm ☐ Punkte

Wenn ich ihm 0 Punkte gebe, erhalte ich von ihm ☐ Punkte

---

Haben Sie alle fünf Kästchen ausgefüllt?

Dann stecken Sie bitte das Blatt in den Briefumschlag und überreichen Sie den **verschlossenen** Umschlag dem Interviewer!

**Vielen Dank fürs Mitmachen!**

TNS Infratest Sozialforschung Deutsches Institut für Wirtschaftsforschung

# Leben in Deutschland

## Verhaltensexperiment „Geben und Nehmen"

Zwei Personen,
die sich nicht kennen,
treffen jeder eine Entscheidung
über die Verwendung von Geld
und erzielen zusammen ein Ergebnis.

### Spielregeln: So wird's gemacht!

Es gibt zwei Teilnehmer.
Jeder Teilnehmer erhält einen Anfangsbetrag von 10 Punkten.

Beide Teilnehmer können ihre 10 Punkte
- zwischen sich und der anderen Person aufteilen oder
- ganz für sich behalten oder
- ganz an die andere Person weitergeben.

Für jeden Punkt, den Sie behalten, bekommen Sie selbst 1 €. 

Für jeden Punkt, den Sie weitergeben, bekommt die andere Person 2 €. 

Umgekehrt gilt:
Für jeden Punkt, den die andere Person an Sie weitergibt, bekommen Sie 2 €

**Dies bedeutet für beide Teilnehmer:**
**Behalten: 0 Punkte = 0€ / 1 Punkt = 1€ / 2 Punkte = 2€ / 3 Punkte = 3€ *usw. bis* / 10 Punkte = 10€**
**Weitergeben: 0 Punkte = 0€ / 1 Punkt = 2€ / 2 Punkte = 4€ / 3 Punkte = 6€ *usw. bis* / 10 Punkte = 20€**

**Sie sind Teilnehmer 2, entscheiden also als Zweiter.**

Teilnehmer 1 hat begonnen, er hat schon entschieden, wie viele Punkte er behält bzw. an Sie weitergibt. Das Ergebnis finden Sie auf der Rückseite.

*Teilnehmer 1 ist eine **andere Person** als letztes Jahr!*

Jetzt sind Sie an der Reihe, das heißt, Sie entscheiden, wie viele von Ihren 10 Punkten Sie an die andere Person (Teilnehmer 1) weitergeben und wie viele Sie behalten.

Aus beiden Entscheidungen zusammen errechnet sich der Geldbetrag, den Sie erzielen. Diesen Geldbetrag werden wir Ihnen per Verrechnungsscheck zusenden. Ihre Teilnahme ist freiwillig. Der Rechtsweg ist ausgeschlossen.

**Um Ihre Punkte einzutragen, drehen Sie das Blatt bitte um.** ➡

- 1 -

## Entscheidungsblatt für Teilnehmer 2

**Sie sind Teilnehmer 2**, das heißt Teilnehmer 1 hat seine Entscheidung bereits

getroffen und hat Ihnen ☐ Punkte gegeben.

***Teilnehmer 1 ist eine andere Person als letztes Jahr!***

Wie teilen Sie die **10 Punkte** auf?

Das heißt, wie viele Punkte **(von 0 bis 10)** geben Sie an Teilnehmer 1 weiter und wie viele behalten Sie?

*Ich gebe* ☐ *Punkte weiter*

*und behalte* ☐ *Punkte*

zusammen also 10 Punkte.

Haben Sie die beiden oberen Kästchen ausgefüllt?

Dann stecken Sie bitte das Blatt in den Briefumschlag und überreichen Sie den **verschlossenen** Umschlag dem Interviewer!

**Vielen Dank fürs Mitmachen!**

- 2 -

CHAPTER

# FIVE

# CONCLUDING WORDS ON TRUST AND RESEARCHING IT

In the following I first point out what are, in my opinion, the most important results of the essays in this thesis. Following this, I reflect on some insights which were gained less from the data and in relation to the original questions asked in this dissertation, but rather from the processes entailed, such as data elicitation and analysis and reporting. In particular, I comment on the problems and advantages associated with implementing experimental approaches on a nationally representative scale. I continue to address that perspective which is associated specifically with economists' ways of thinking, and on how it shapes and affects data analysis and interpretation.

Let me start with the first chapter, 'Can We Trust the Trust Game?'. In this essay, we came to the conclusion that yes one can indeed trust the trust game - it trustworthily does what it is expected to do. We demonstrated that the game is robust to a number of interferences. Still in the same chapter we also connected survey and experimental measures of trust by showing where they overlap and where not. The bottom line here is that the experimental measure specifically measures one dimension of trust, namely trust in strangers. The survey measure is more strongly correlated to expectations than to actual trusting behaviour which suggest that the survey measure mainly measures the expectation-part of the decision to trust. This is not very surprising since the wording of most survey measures of trust induces people to rate statements about expected trustworthiness. Nevertheless, the precise calculation of the dimensions shared by both survey and experimental measures of trust, as well as of what distinguishes them, is an important contribution to the growing methodological literature on trust.

Having dealt with some mostly methodological aspects of trust, the following two chapters turn to specific empirical questions. In the second chapter 'Decomposing Trust:

Explaining National and Ethnic Trust Differences' we showed that at the time of our data collection (2005), the level of trust was 20% higher in the United States than in Germany. As the difference is rather striking, we gave it a name: the trust gap. The trust gap can be fully explained by the facts that U.S. Americans exhibit lower risk and betrayal aversion and are more optimistic about their countrymen's trustworthiness than Germans are. This result is interesting per se, but equally interesting is that aggregate risk and social preferences differ quite substantially between U.S. Americans and Germans, as we went on to demonstrate. As it is generally accepted that risk and social preferences are important for many decisions people take, it is surprising that measures for these preferences are not included in large scale surveys such as the World Values Survey or the General Social Survey. To the best of my knowledge there is no study except ours that compares risk preferences across countries on a representative basis. Thus the relevance of this result is not to be underestimated.

Whereas chapter three introduced this novelty of studying and explaining trusting behaviour representatively across countries, it does remain a somewhat static approach. In the following chapter I attempted to rectify this by focusing on the dynamics of change across time. In the fourth chapter, 'Trust and Betrayal: a Vicious Circle', we showed that the experience of untrustworthiness decreases the extent to which we are prepared to trust. Until now, little is known about the temporal dynamics of trusting behaviour. On the basis that trust in strangers is important for economic and social relations, I study the forces that drive the changes in individuals' levels of trust. From theory I expect that positive and negative experiences change trusting behaviour. However, no empirical study has yet investigated whether and how this happens. In this paper, I am able to establish a strong causal link from past experience of untrustworthiness to reduced trusting behaviour, measured by a repeated trust game over the period of one year. I determine that this causal link mainly functions by way of changed beliefs, and is moderated by social preferences. Experience of trustworthiness, in contrast, does not alter trusting behaviour. Furthermore, I investigated if an increase in the experience of trust promotes trustworthy behaviour. I show that there is indeed a causal link here, too. I conclude that negative experience in relation to trust might kick-start a vicious circle thus eroding overall levels of trust. Further research is needed for better understanding of the factors that increase trusting behaviour.

In experimental economics one usually conducts experiments using student volunteers as participants. The advantages to this approach are that they usually are smart, know how to use a computer, understand even complex instructions, and have low opportunity costs. The disadvantage is, however, that the results from these experiments are not generalisable to the whole population and thus conclusions from theses experiments

might only hold in the very limited population of students, thus limiting the relevance of such research.

Selecting a nationally representative sample for participating in an experiment obviously solves the problem of generalisability. But the advantages do not end here. A further advantage is that the sample is much more heterogeneous in terms of socio-economic characteristics such as income, education, age, and life experience in general. As there is more variance in the data, the exploration of the interesting relationship of socio-economic characteristics with behaviour becomes feasible. These advantages, however, come at a cost. The main disadvantages of using a representative sample are the mirror image of the advantages of a student sample. A nationally representative sample will necessarily include less educated participants, who might find it difficult to understand the rules of the game. Some might not even be able to read or do not speak or read the national language. Furthermore, lots of people have little to no computer literacy. Sitting them down in front of a computer screen with z-Tree in front of them might have decidedly problematic and data-distorting effects, such as for instance scaring them. Finally it is very expensive to collect data on a representative scale[1]. In order to counteract these problems, any project that aims to conduct experiments on a representative scale - as ours do - must devise and use a very simple design, which is easy to explain to participants.

Collecting representative data on your own is complex and costly. From a pragmatic point of view, integrating your experiment into an existing panel study has the advantage of outsourcing the sampling and collection of the data. However this comes with a price. Panel organisers are very careful about not losing panel participants, thus they will avoid anything that might annoy or anger their respondents. In the experiment they might see such a risk. This has the result of restricting the integrated experiment to simple, straightforward and quick designs as anything else will fail to gain the organisers' approval. One thing that would clearly be difficult to implement is to play a game with multiple periods. I combined the experiment with the German Socio-economic Panel Study, where they were very open to integrating experimental methods into the panel. However, it took quite some effort to convince the governing board to agree to this 'experiment'.

As I pointed out above, the design of the experiment has to be simple in order to be understandable for everyone. An example we encountered was that hypothetical questions are difficult to ask, because lots of people have difficulties imagining a non-existent situation. This has consequences for the elicitation of expectations, which can

[1] At this point I would like to reemphasise my gratefulness to my supervisor Ernst Fehr for organising the financing of the implementation of the trust game as part of the SOEP survey.

only be done in a very simple way. Asking people how certain they are about their expectation is almost impossible to do successfully. It might appear to be more pragmatic to simply go ahead and implement standard games on representative scales, or include complex and cognitively demanding questions without much reflection, but proceeding in this way would have consequences for the interpretability of the resulting data. Ignoring the cautions about the complex and cognitively demanding designs for experiments on a representative level is likely to cause increases in noisy behaviour. In other words, participants might simply decide something randomly in order to bring the experiment to an end. Noisy behaviour in student samples is maybe less of an issue but still a possible driving force in behaviour, which is often not taken into account when analysing data. The reasons for not taking this into account may lie in the specific view economic theory has on individual's decision making.

From a typical economist's point of view, an individual's decision or behaviour is based on rational reasoning which aims to maximise utility. A certain amount of random decisions is usually expected and considered not to be a problem for interpreting, as it simply increases the noise in the data. However, depending on the experimental design, random behaviour can in some cases result in seemingly structured patterns, or correlations which appear convincing but are in fact spurious and simply an effect of the design. In other words, a correlation is observed which would not exist if no-one behaved randomly. This was a relevant insight I gained while crunching through the data reported in chapter 4. If a first-mover in this situation fully trusts the trustee and transfers the whole endowment of 10 points, the trustee is by definition only taken to be trustworthy if he/she also transfer all of the 10 points back to the first-mover. This means that if the second-mover decides randomly about the back-transfer, the chances that he/she is trustworthy is $\frac{1}{11}$. In contrast, if the first-mover transfers 5 points, the likelihood of observing trustworthy behaviour, given random decisions, increases to $\frac{6}{11}$. Thus simply the participation of some randomly behaving second-movers would create a negative correlation between trust and trustworthy behaviour in the data, or reduce a possibly positive relationship and render it insignificant. Unfortunately, this problem is often ignored when designing and analysing experiments. In some cases, it could be solved by adapting the design, but being unaware of the problem leaves it to chance if the design bypasses the problem or not. In consequence, the problem often needs to be addressed on the level of analysis. The simple assumption, widespread among economists, that people maximise their utility unfortunately can prevent researchers from seeing this possible bias, thus causing erratic findings and obscuring possible real effects.

Another example where the standard assumptions of rationality and selfishness influ-

ence the design of experiments, is how expectations are normally measured and analysed in experiments on social preferences. Obviously, the trust game is my example of choice. Here, first-movers are asked what amount they expect back from the second-movers or sometimes what they expect the return ratio to be. This reported estimate is then used to explain first-mover behaviour. Leaving aside endogeneity problems, such explanations may only actually be meaningful for participants who maximise their income. In contrast, the reported expected back-transfer might have totally different influences on behaviour for people who try to minimise inequality. I started to analyse expectations as everyone else did. Soon I started to realise that for inequity-averse first-movers it would not be reasonable to expect a positive relation between trust and expected back-transfer such as would indeed be expected for income-maximising first-movers. Thus, I took this into account when using expectations as explanatory variables (section 3.4). I consider this a simple but important methodological insight gained from this thesis. In contrast, to the best of my knowledge, no other paper apart from ours allows expectations to have differentiated effects on behaviour for different types of people. Thus, while designing and analysing experiments one should always bear in mind the limitations of economic theory and be aware of other possible explanations, predictions and theories.

Overall, the main insights to be derived from this thesis are not least on a methodological level. This includes data elicitation techniques and their advantages and problems as well as approaches to data analysis where particular issues necessitated devising new procedures. This said, the empirical results are obviously at centre stage: The trust gap between U.S. Americans and Germans can be fully explained by deploying concepts of economic theory; and mistrust and betrayal are related in the form of a vicious circle. Whereas I hope to have contributed to some key questions within the field of the microeconomics of trust, many other questions have not been addressed, and the field offers plenty of possibilities for future investigations.

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